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ABSTRACT

This volume presents the results of a data analysis of an employer survey to investigate uses of subsidies to provide jobs and training for unskilled and inexperienced workers and to examine the impact of those uses on employers. Chapter 1 introduces the subsidy programs and the data set and tabulates how participation in each program varies with industry and establishment size. Chapter 2 examines which employers are familiar with the programs and addresses employer awareness of program existence. By studying determinants of firm participation in the program, chapter 3 explores reasons why so few employers are familiar with a program participant. Chapter 4 examines the impact of these programs on employment levels of participating firms. Relevant research on employment and training subsidies is reviewed in chapter 5, and changes in program design are recommended. Chapter 6 analyzes determinants of the employer's investment in selecting a new employee--the number of applicants interviewed and the number of hours spent recruiting and interviewing for a position. Rates of resignations and dismissals are studied in chapter 7. Chapter 8 examines the amount of training provided to new workers and the resulting productivity growth. The determinants of wage growth are analyzed in chapter 9. The survey instrument is appended. (YLB)

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SUBSIDIZING ON-THE-JOB TRAINING

An Analysis of a National Survey of Employers

Studies in Employment and Training Policy: 1

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FOREWORD

This volume is part of the final report on research supported by a contract with the Employment and Training Administration of the U.S. Department of Labor. The work was initiated in 1979 at the Institute for Research on Poverty, University of Wisconsin, Madison. In July 1981, the project's principal investigator took a position as associate director of the Research Division at the National Center for Research in Vocational Education, The Ohio State University, thus bringing with him one component of this research effort, while another component was continued at the Institute for Research on Poverty under subcontract from the National Center.

This research would not have been possible without the cooperation and assistance of 5,919 employers who so graciously responded to our telephone interview. We greatly appreciate the time and the insights that these very busy men and women contributed to the study.

The project is also indebted to the many employers who assisted in the design of the interview instrument. In this regard, special thanks are due to Clifford Roe, Supervisor of Salaried Union Relations and EEO Administrator (retired), Buffalo Divisions, Westinghouse Electric Corporation; and William J. Dennis, Research Director, National Federation of Independent Business. Wilson S. Johnson, President of the National Federation of Independent Business was very supportive of the study, and graciously provided a letter of introduction that we sent to all the employers selected for an interview.

Thanks are extended to Joe Meskey and Nancy Williamson for their capable programming and data base preparation in the course of the project. The manuscript was edited by Janet Kiplinger and typed by Vera Mueller, Jacque Masters, and Cathy Jones.

Robert E. Taylor
Executive Director
The National Center for Research
in Vocational Education

PREFACE

The work contained in this volume is one of the products of DOL-funded research to study the Labor Market Effects of the Employment Opportunity Pilot Projects. The Employment Opportunity Pilot Projects were a large scale demonstration of the Carter Administration's Welfare Reform Proposals. The program was cancelled in the Spring of 1980 but baseline data had already been collected on over 5,919 employers. This volume reports on our analysis of this data.

I began work on this project in 1979 while at the Institute for Research on Poverty at the University of Wisconsin, Madison. In July 1981, I moved to the National Center for Research in Vocational Education, The Ohio State University, where I continued my involvement with this research, the prime contractor of which was Stanford Research Institute International. I would like to thank Gene Smolensky, Director of the Institute for Research on Poverty, and Robert E. Taylor, Executive Director of the National Center for Research in Vocational Education, for their support and encouragement of this effort.

In addition, I would like to thank the staff at WESTAT, Inc. who were responsible for designing the sampling frame and conducting the telephone survey. Steve Dietz, Diane Ward, Mike Shea, Carmen Vincent, Joe Waksberg, Adam Chu, and Jack Ogus did an outstanding job in this portion of the project. Their enthusiasm and professionalism were responsible for the high response rates obtained and the high quality of the data set that was generated.

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I would also like to thank my colleagues at the Institute for Research on Poverty, SRI International, Purdue University, and the National Center for Research in Vocational Education for their helpful comments and critiques of early drafts of many of this report's chapters. I am especially indebted to John Barron, William Dunkelberg, John Gardner, Peter Gottschalk, Mike Keeley, Stan Masters, Mark Meitzen, Mark Montgomery, Jeanette Schrier, Grace Schubert, Stanley Stephenson, Richard Toikka, and Arlene Waksberg.

Finally, I want to thank my wife, Marcie, and my two sons Michael and Matthew for putting up with the disruptions of their lives that this volume necessitated.

John Hillman Bishop
August 1982

EXECUTIVE SUMMARY

The persistence of high unemployment rates amongst minorities and youth in the face of tight labor markets for skilled workers and accelerating wage/price inflation has led economists and politicians to search for new ways to stimulate employment and training opportunities for inexperienced and disadvantaged workers.

Programs have been established to induce the private sector to create additional jobs and to provide training for unskilled and inexperienced workers. In 1980 four such programs were in operation: the Targeted Jobs Tax Credit (TJTC), the WIN tax credit, CETA's On-the-Job Training (OJT) contracts, and WIN-OJT contracts. Since 1979 the Targeted Jobs Tax Credit has provided, to employers outside the personal service sector, a tax credit for hiring certain categories of workers. In 1980 these included: high school students in cooperative education programs, economically disadvantaged youth (ages eighteen to twenty-four), veterans, ex-convicts, Supplementary Security Income and general assistance recipients, and the handicapped. At the end of 1981 non-disadvantaged cooperative education students were dropped from eligibility for TJTC. The credit amounts to 50 percent of the first $6,000 of wages per employee for the first year of employment and 25 percent of such wages for the second year of employment.

The other programs have been in existence in one form or another since the early 1970s. Employers hiring recipients of Aid to Families with Dependent Children (AFDC) may now receive a WIN tax credit of 50 percent of the first year's wages and 25 percent of the second year's. During the summer of 1981, the WIN tax credit was merged into TJTC. CETA-OJT is a program whereby local prime sponsors contract with private employers to hire and train workers referred by CETA. The contract generally specifies that one-half of the wages paid during the first six months be reimbursed by CETA. The WIN-OJT program is very similar to CETA-OJT.

In order to learn more about employer utilization of these subsidies and their impacts on employers, the U.S. Department of Labor contracted with WESTAT, Inc., of Rockville, Maryland, to conduct a thirty-minute telephone survey with each of more than 5,300 employers. This monograph makes use of data from interviews with 4,832 establishments with one or more employees in December 1979. The respondents are a stratified random sample of establishments that pay unemployment insurance taxes from thirty separate locations around the country.

This volume presents the results of an analysis of this data set. The first five chapters examine participation in and the effects of the employment and training subsidies that are currently in operation. Chapter 1 introduces the reader to the subsidy programs and the data set and tabulates how participation in each subsidy program varies with industry and the size of the establishment. Chapter 2 examines which employers are familiar with the programs

and attempts to understand why most employers are unaware of the existence of these subsidy programs. Chapter 3 explores the reasons why so few of the employers that are familiar with a program participate in them by studying what determines whether a firm participates in the program. Chapter 4 examines the impact of these programs on employment levels of the firms that participate in them. Chapter 5 reviews all the relevant research on employment and training subsidies and makes recommendations for changes in the design of the programs.

The second part of the volume analyzes the context in which employment and training subsidies operate: the hiring, training, and wage-setting policies of the firm. The determinants of the employer's investment in selecting a new employee--the number of applicants interviewed and the total number of hours spent recruiting and interviewing for a position--are analyzed in chapter 6. Rates of quitting and dismissals are studied in chapter 7. The amount of training that is provided to new workers and the productivity growth that results is examined in chapter 8. The determinants of wage growth are analyzed in chapter 9.

The first three chapters of the volume focus on the determinants of employer participation in these programs. Firms that are ignorant of the existence of a program cannot participate in it, so a separate analysis was conducted of "familiarity" with the programs and participation given familiarity. A theoretical model of a knowledgeable firm's decision to participate in a targeted employment and training subsidy was developed. Firms were assumed to participate if the benefit of participating (the dollars of subsidy received) exceeds the costs. Learning about the program and establishing channels for identifying and recruiting eligible workers are an important part of the costs of participating. These costs are unrelated to the number of workers subsidized so costs of participation do not rise proportionally with the number of workers for whom one receives subsidy. The benefits of participating are more likely to exceed the costs at firms (a) that have many openings that eligible workers might fill, (b) that have low costs of identifying, certifying, and employing eligible workers, and (c) that receive large subsidies for each eligible workers. When the net benefits of participation are high the incentive to invest in information is greatest so determinants of participation given knowledge are likely to also be the determinants of knowledge. The other important determinant of familiarity is likely to be the costs of learning about the program.

The effectiveness of employment subsidies in creating jobs is severely hampered by simple ignorance of the programs. Only 17 percent of all employers were familiar with TJTC, and only 23 percent were familiar with WIN. Slightly more than half of the firms in the EOPP Survey were familiar with the CETA-OJT programs, and slightly less than half were familiar with the New Jobs Tax Credit. Familiarity with these programs was found to be positively associated with two measures of the likely benefit of participating--size of the establishment and proportion of the work force in white-collar occupations--two indicators of a low cost of learning about the program--membership in local business organization, previous receipt of other subsidies--and an indicator of outreach by local program administrators--the proportion of all employers in the site who learned of WIN or CETA from a government representative.

Knowledge of CETA-OJT is greatest in rural sites with a high 1978 unemployment rate and high proportion of minorities. This reflects the funding formula of the CETA program and the tendency of rural administrators of CETA to focus their dollars on OJT because classroom training is costly and inefficient where distances are great and labor markets are thin.

In our view, the primary explanation for geographic variation in familiarity with these programs is variation in local promotional efforts (only some of which were measured by our outreach variable). From this we derive the hopeful conclusion that while ignorance was, in 1980, the major barrier to use of these programs, effective promotional efforts have and can overcome this barrier.

Only a few of the employers that are familiar with these programs participate in them. Thirteen percent of those familiar with TJTC participated in it. Three percent of those familiar with the WIN and CETA-OJT programs participate in them. The employers that do participate tend to be the ones with many unskilled job openings. Participation was positively related to establishment size, the proportion of the work force in unskilled jobs, and the rate of growth of employment. Firms that can, without great difficulty, fire employees who do not perform well are more able to take risks when hiring. They are considerably more likely to participate than firms that cannot fire employees easily. Firms that use one subsidy program are more likely to use other subsidy programs.

Outreach by program administrators was found to have a significant impact on both knowledge and participation. Employers that have been contacted personally by a government representative about the program are considerably more likely to participate.

Some states have been very successful in marketing TJTC. At the time of our interview, Georgia, South Carolina, and Alaska had participation rates that were ten times those of California. The willingness of firms to participate in these programs does not vary appreciably from state to state; what does vary are the policies and commitment of the local administrators of the program. It is the lack of the local employment services' commitment to promoting the TJTC program in most parts of the country that is responsible for the low participation. One Oregon employer found his local employment service office ignorant of TJTC and uncooperative. He claimed they were not even set up to certify the eligible workers that he found and hired; he had to go down to the office to teach the staff there how to certify someone.

The other major finding is that the nonpecuniary cost of participating is significant and largely fixed in nature. In our data this results in large firms with many unskilled jobs being more likely to participate. Once a firm has learned how the program works and has developed channels for recruiting eligible workers, the costs of hiring additional eligible workers fall. The result is that while most firms do not participate, some of those that do participate hire large numbers of eligible workers. There is, for instance, a janitorial service company in an eastern city that is alone responsible for 1 percent of the entire nation's WIN tax credit claims in the 1978-1979 period.

Hiring of subsidy-eligible workers is highly concentrated in a few firms. Even though less than 1 percent of all workers are subsidized, the typical subsidized worker is working at an establishment at which 14.6 percent of the firm's employees are subsidized. If participation in these programs is to increase, it is important to keep the marginal costs of hiring and certifying additional workers low and, if possible, to make them lower. It may be that the most efficient way to reduce the structural unemployment of welfare recipients and disadvantaged youth is to encourage what seems to be a tendency for certain employers to specialize in hiring and training this disadvantaged population.

Chapter 4 of the volume examines one of the many possible indicators of the impact of these subsidy programs on the economy--their impact on the employment levels of participating firms. It was found that when employers were asked what impact these programs had upon their employment levels, 25 percent of our respondents reported that they had increased employment. The size of the reported employment increase was roughly one-fifth of the total number of subsidized workers hired (including those hired by firms that reported no increase in employment).

The econometric estimates of the impact of these subsidy programs are even more favorable. About three-quarters of all private wage and salary employment is in establishments with twenty or more employees. Only a slightly smaller proportion (70 percent) of all subsidized employees work in establishments that have more than twenty employees. In these establishments our preferred two-stage least squares (2SLS) model obtains point estimates of the impact of hiring subsidized workers that imply that extra subsidized employees produce an approximately one-for-one increase in the establishment's total employment. The ordinary least squares (OLS) estimates of the same model also imply a large impact: the change in employment per subsidized worker was .64 for establishments with twenty-one to one hundred employees and .235 for establishments of greater than one hundred employees.

These OLS and 2SLS results are our best point estimates of the impact of the subsidy programs on the employment levels of the subsidized firms. If anything, they are biased down because they cover only the final five-and-a-half months of 1979. The analysis of 1980 employment growth suggests that there was no tendency for these 1979 gains to be lost in 1980. The problem with these results, however, is that the confidence intervals (especially those that use two-stage least squares) of these point estimates are very wide. In the 2SLS models the hypothesis that there is no subsidy program impact cannot be rejected. In the OLS models we may reject the hypothesis of no impact only for the twenty-one to one hundred employee establishments. As a result we cannot draw conclusions from this analysis with any great confidence. Unless the true impacts had been unreasonably large, our data set with its small number of participating firms could not have produced an estimate of subsidy impacts on the employment that were significantly different from zero. All we can say is that the subsidy programs seemed to be having a significant impact on the employment of participating establishments with more than twenty employees in 1979, but that these results could have been a statistical fluke.

Another reason for treating these findings with caution is their partial equilibrium nature. They do not tell us much about the general equilibrium effects of these programs. On the one hand, firms compete with each other in product and labor markets, so expansion by one may induce another firm to contract. This causes general equilibrium effects to be smaller than the partial equilibrium effects we estimated. On the other hand, if the disadvantaged worker who is hired because of the subsidy would otherwise not have been able to get a job (because of the minimum wage or some other imperfection in the market) and if the less disadvantaged worker who is displaced does find another job because he/she is part of a labor market in which wage rates adjust up and down to equilibrate demand and supply, total employment in all firms may expand even if participating firms do not increase their employment.

Chapter 5 reviews the lessons that can be learned from U.S. experience with employment and training subsidies. There is a dramatic contrast between the take-up rates of subsidy schemes targeted on particular people--TJTC, WIN, and CETA-OJT--and schemes that offer aid for general expansions in employment like NJTC. In its second year of operation more than 1.1 million firms, more than 30 percent of all the nation's employers, and more than half of the eligible firms received a New Jobs Tax Credit. In contrast, less than 20 percent of the private employers that hire WIN eligibles obtain certification of their eligibility and less than 10 percent of the employers that hire TJTC eligibles obtain certifications.

Studies of the impacts of these programs were also reviewed. NJTC's impact on employment has been examined in three studies and all have concluded it had a significant impact. One study found it reduced prices as well. There is a possibility, however, that the employment and output expansion induced by NJTC caused an acceleration in the rate of wage increases even while price increases were slowing down.

Very little is known about the impact of the targeted programs. The previous chapter presents evidence that a large share of the subsidized employees in establishments with greater than twenty employees were net additions to the company's employment rolls. The large confidence intervals on the estimates, however, make this evidence inconclusive. The targeted programs were designed to influence hiring decisions. It has been charged, however, that since more than half of the certifications in the TJTC program were obtained long after the hiring date, only a few of these decisions were influenced by the availability of the programs.

The major drawback of the targeted programs is their low take-up rate. Take-up is low because of ignorance of the programs' existence and provisions, the stigma attached to being members of most of the target groups, and the complicated eligibility rules and the consequent necessity of government certification of worker eligibility (rather than employer certification with audit).

Our review of past experience suggests that a subsidy of private sector employment will reach a scale and cost-efficiency sufficient to make a real dent in structural unemployment, only if--

1. employers are able to simply certify their own eligibility,

2. the behavioral response desired of employers is obvious and simple for them to implement,

3. all or almost all employers are eligible (otherwise the result is a redistribution of who employs whom),

4. a target group is selected that is in excess supply and/or has high elasticities of labor supply (note: if targeting is imperfect it is better to include too many than to include too few in the target group),

5. the target group is defined by a nonstigmatizing criteria that is visible to the employer (a characteristic of the job such as the wage rate is better than characteristics of the worker),

6. it is marginal--paid for increases in employment above a threshold like NJTC.

A simple scheme that meets all of these requirements would include--

- a tax credit of $1.00 per hour for every hour the firm's 1982 total hours worked by all employees including salaried management exceed this same number for 1981, subject to a limitation if wage rates increase more than 6 percent;

- increases in 1983 total hours worked over the higher of 1982 or 1981 hours worked would also receive a $1.00 an hour subsidy subject to a takeback if the wage rate has increased more than 11 percent above 1981 level.

This tax credit scheme transmits a very simple message to employers:

- Increase hours worked.

- Slow down wage increases.

- Focus the employment expansion on your lower wage, lower skill employees.

- Marginal costs of extra output have decreased by as much as 25 percent so cut prices and increase output.

The reduction of the subsidy if the average wage increase exceeds the threshold is an important targeting device. Because, if the firm has exceeded the wage standard and the subsidy reduction rate is 20 percent, hiring an extra employee in a job that pays $5.00 per hour less than the firm average gives the firm a tax credit of $2.00 rather than $1.00 an hour.

The final four chapters of the book examine the context in which subsidy programs must operate. In chapter 6 we investigated the role of employer search as an explanation for differences in hiring cost across employers. The expected total cost of hiring employees is the product of three separate components--the cost of interviewing one applicant, the expected number of applicants interviewed before making an offer, and the expected number of offers needed to fill one position. A theory was developed that viewed each of these components as choice variables. The empirical analysis focused on the determinants of the number of applicants interviewed per acceptable applicant. A major finding is that positions involving a greater amount of specific training, particularly that training provided by personnel and supervisory staff, are ones for which the employer is more careful about who is hired. The number of applicants interviewed per acceptable applicant and total staff time spent recruiting, screening, and hiring for the position are higher for jobs that require a great deal of training. Employer search is more extensive and hiring costs are greater at large firms, at firms with a large flow of applicants, for positions that require high levels of education, and for professional and clerical positions.

Chapter 7 is an empirical analysis of the determinants of permanent job separations for recently hired, low skilled workers. The theoretical model that provides a guide for the development of testable hypotheses is a job-matching model in which firms and workers search for a suitable employment match under conditions of imperfect information. In general, the hypotheses derived from this model are concerned with how the probability of separation is affected by the costs of separation, the quality of the employment match, and the availability of alternative matches.

The continuous time RATE model developed by sociologist Nancy Tuma was used to model permanent job seperations. RATE is a maximum likelihood procedure that uses duration data to estimate the conditional probability of leaving a particular state given the length of time spent in that state. The study found that the probability of a worker quit is inversely related to the size of the establishment, the extent of firm unionization, the amount of prehire screening done by the firm, wages, and the worker's age. The results of the discharge equation were rather poor, as only a few of the coefficients were statistically significant.

The finding that unionism reduces quits lends support to the Freeman "exit-voice" hypothesis which states that, in addition to the monetary gains provided by unionism, the union grievance machinery provides workers with a "voice" option of resolving problems in the workplace as an alternative to the "exit" option of resolving problems by quitting. This study finds that the probability of a worker quitting in a highly unionized firm is 30 to 38 percent lower than the probability of a worker quitting in a nonunion firm.

Other notable results from the quit equation include the effects on the probability of quitting of (1) the amount of prehire screening done by the firm, (2) the top wage of the job slot the worker was hired in, and (3) the age of the worker at the time of hire. First, the number of hours spent by

the firm screening potential workers before hiring someone negatively influences the probability of quitting. Consistent with the job matching framework, this result implies that firms which hire workers on the basis of better information will be able to form better matches and, hence, lower voluntary turnover. Second, the higher the top wage the worker can expect, the lower the probability of quitting. This result simply implies that workers will be less willing to quit higher paying jobs. Third, it is found that older workers are less likely to quit. This is the case because older workers have a shorter working life in which to capture the returns than do younger workers and because younger workers are more likely than older workers to be engaged in job shopping in an effort to discover what labor market talents they possess.

The restriction of the sample to recently hired, low skilled workers means that inferences cannot be drawn from this study for the entire population of workers. However, in the terminology of the segmented labor market literature, this concentration on low skilled workers allows analysis in this study of some of the differences between jobs and workers in the primary and secondary labor markets. The primary labor market is characterized by internal job ladders--i.e., a sequence of jobs with the same employer in which the skills for a job on one rung of the ladder are developed at jobs on lower rungs of the ladder--and recently hired, low, and semiskilled workers will be found on the lower rungs of these ladders, which are often referred to a ports of entry to the internal labor market. In essence, the firm is creating its own supply of skilled labor via the job ladder, and therefore, an incentive exists to encourage stable, relatively permanent employment relationships in order to ensure a predictable supply of skilled workers. Conversely, employment in the secondary labor market is characterized by jobs that require little skill for satisfactory performance. Furthermore, these jobs tend to be "dead-end" jobs in that they are not ports of entry to an internal labor market. Therefore, little incentive exists for the secondary employer to encourage stable employment relationships, and hence, greater worker turnover is tolerated. For example, firm size and percent of firm's employees who are craftworkers can be interpreted as proxies for internal job ladder development since larger firms are more likely to offer training and advancement opportunities to workers, thereby developing many of these skilled workers internally. Thus, the negative relationship found between these variables and the probability of quitting tends to support the view that employment is more stable in the primary labor market.

Chapter 8 undertakes a comprehensive analysis of job turnover, and training and productivity growth of newly hired, unskilled and semiskilled workers. The theoretical perspective that is applied to this analysis merges Becker's theory of on-the-job training (OJT) with Jovanovic's job matching theory. A unique element of the data set we analyzed is its measurement of the employer's opinion of the productivity of a recently hired worker at two separate points in time--two weeks after being hired and six or more months after being hired (which was the time of the survey interview). The average productivity score of the workers who were still working at the firm six or more months after being hired was 42 percent higher than the average score of employees with only two weeks of tenure. Average scores rise (a) because training

increases the productivity of new employees (both those who stay and those who leave) by 29 percent and (b) the individuals that stay with the firm are more productive than those who leave. This tendency for the stayers to be more productive than leavers and hence for the average productivity of the employees that remain to rise with tenure independent of training is strongest in small nonunion establishments. These findings provide strong support for both the OJT and Jovanovic's sorting explanation of wage and productivity growth.

Separate probit models were estimated predicting voluntary and involuntary turnover. It was found that the determinants of voluntary turnover were very different from the determinants of involuntary turnover. Being female and having more years of schooling increased the likelihood of quitting but decreased the likelihood of separating involuntarily. Higher relative wage rates, significant amounts of training, and high proportions of the work force in craft occupations were associated with significantly lower quit rates but slightly higher rates of involuntary separation. Unionized firms had considerably lower quit and dismissal rates but considerably higher layoff rates. Eighteen of the twenty-three coefficients estimated had opposite signs in the two equations.

Data on training investments were obtained by asking how many hours were spent in the first month of _____'s employment orienting and training him/her (a) by personnel and supervisory staff and (b) by coworkers who were not part of management. In the first month, management spent an average of 20.8 hours training the new employee and coworkers spent 14.4 hours. The total value of time management staff and coworkers spent training a unskilled or semiskilled employee in the first month was equivalent to at least one-third the first month's wages. Workers with previous useful work experience needed and got less training. New employees with a great deal of formal training need less training but seemed to get more because they were being taught more and higher level skills. Males received more training from management than women. The greatest amount of training was offered by large establishments with high proportions of work force in white-collar or craft occupations. Unionization had no effect on training time investments.

The examination of the determinants of productivity growth yielded some very important findings. We would conclude that investments in training time have large statistically significant positive impacts upon the change of the productivity index over time. Under the assumption that the productivity index is a proportional transformation of the true relative productivity, the rates of return to these investments were calculated and found to be quite high. The yearly rates of return were estimated at 54 percent for training by management and 40 percent for training by coworkers. When initial productivity and training investments are controlled for in a structural model of the learning process, education, previous useful experience, and being female were associated with somewhat higher rates of productivity growth. These results suggest that the rate at which a new job is learned is greatest for women, for more educated workers, and for workers with significant amounts of previous useful job experience. Being younger is not associated with being a faster learner. The only characteristic of the employer that had a statistically significant effect on the rate of learning was establishment size.

According to Becker's theory of OJT, workers induce employers to offer costly, on-the-job training by being willing to accept a lower wage when the job offers training in general skills. The minimum wage law prevents wage rates of jobs offering general training from falling low enough to finance the employee's share of training costs. This induces some firms to avoid hiring inexperienced workers and other firms to redesign jobs so that the skills learned (if any) are not useful to other employers. Consequently, a number of economists have hypothesized that the minimum wage discourages on-the-job training of young and inexperienced workers. If this hypothesis is correct there should be a curvilinear relationship between the nominal wage and training investments when the qualifications of the worker are held constant. Training investments should be low, both in high wage jobs and in minimum wage jobs. Tests of this hypothesis in the models of training investments and productivity growth found considerable support for the hypothesized negative impact of the minimum wage.

The final issue to be addressed in our review of chapter 8 is whether and how the subsidized workers were different from unsubsidized workers. Only a few of the workers were known to be eligible for a TJTC or WIN tax credit, and the resulting sample was too small to produce significant coefficients on dummies for these programs. The special sample of CETA-OJT workers made possible a reasonable estimate of how these workers were different. The program is targeted on the disadvantaged, and as one might anticipate, their quit rates were higher. Furthermore, their rate of productivity improvement was lower than that of other workers who started at the same level and received the same amount of training. They received considerably more training, however, and this, plus the fact that they started out behind other workers, meant that their rate of productivity growth was equal to or better than that of unsubsidized workers.

Chapter 9 analyzes the determinants of wage growth and compares wage growth to productivity growth. The most interesting finding is the observation that rates of wage increase are considerably below rates of growth of productivity net of training cost. The starting wage (adjusted for inflation) is typically about 89 percent of individual's current wage. Productivity in the first month, however, is 71 percent of the current productivity of the workers that have remained. The costs of the training investments make productivity net of training costs in the first month less than 40 percent of current productivity. This result is consistent with standard OJT and job matching theory only if almost all training is specific and employers pay almost all the costs of specific training. While the second of these propositions is plausible, the first one is not. If one rejects the specific OJT explanation, one is forced into an implicit contract explanation of the discrepancy. It is possible that in return for employer financing of part of the costs of general training, new employees promise to stay for awhile?

Models of wage growth were estimated that contained measures of productivity in the second week and changes in productivity. Wage rates were found to respond to the individual's relative productivity in small establishments but not in large establishments. The elasticity of this response is low, however, under .15. The weakness of the tendency of wage rates to reflect

individual productivity has a number of causes: errors in measurement, the high costs of measuring productivity, random year-to-year variations in true productivity, and the fact that variations in productivity not visible to other firms need not be fully compensated.

Education, experience, age, and being male all have positive statistically significant effects on both the level of the starting wage and the rate at which it grows in the first year or so of employment. The positive effects of age and maleness on wage growth contrast sharply with their lack of impact on productivity growth. Firm characteristics that have a statistically significant positive impact on wage growth are size, proportion of white-collar workers, and proportion of craftworkers. The rate of growth of employment in the local labor market and level of the local manufacturing wage also had positive and significant effects on both the level and the rate of growth of wage rates. Participation in a subsidy program had no impact on either the wage level or its rate of growth.

CHAPTER 1

AN INTRODUCTION TO TARGETED EMPLOYMENT SUBSIDIES AND TRAINING

John H. Bishop

The persistence of high unemployment rates amongst minorities and youth in the face of tight labor markets for skilled workers and accelerating wage/price inflation has led economists and politicians to search for new ways to stimulate employment and training opportunities for inexperienced and disadvantaged workers. One approach has been to create jobs for this group in the public sector. This has not been an effective strategy, however, for (1) although the federal government pays the full cost of each job, the net addition to state and local employment has been only a fraction of the number of subsidized jobs and costs per job generated are high (Johnson and Tomola 1977, Borus and Hamermesh 1978, Bassi and Fechter 1979, Wiseman, 1976), and (2) promotion opportunities in the public sector are limited and jobs must eventually be found for subsidized workers in the private sector. The skills learned in these public sector jobs are often not transferable to private sector jobs.

These difficulties have led to programs whose objectives are to induce the private sector to create additional jobs and to provide training for unskilled and inexperienced workers. At the time of the survey, four such programs were in operation: the Targeted Jobs Tax Credit (TJTC), the WIN tax credit, CETA's On-the-Job Training (OJT), contracts and WIN-OJT contracts. Since 1979 the Targeted Job Tax Credit has provided, to employers outside the personal service sector, a tax credit for hiring certain categories of workers. At the time of the survey these included: high school students in cooperative education programs, economically disadvantaged youth (ages eighteen to twenty-four), veterans, ex-convicts, Supplementary Security Income and general assistance recipients, and the handicapped. At the end of 1981 non-disadvantaged cooperative education students were dropped from eligibility for TJTC. The credit amounts to 50 percent of the first $6,000 of wages per employee for the first year of employment and 25 percent of such wages for the second year of employment.

The other programs have been in existence in one form or another since the early 1970s. Employers hiring recipients of Aid to Families with Dependent Children (AFDC) may now receive a WIN tax credit of 50 percent of the first year's wages and 25 percent of the second year's. During the summer of 1981, the WIN tax credit was merged into TJTC. CETA-OJT is a program whereby local prime sponsors contract with private employers to hire and train workers referred by CETA. The contract generally specifies that one-half of the wages paid during the first six months be reimbursed by CETA. The WIN-OJT program is very similar to CETA-OJT.

In order to learn more about employer utilization of these subsidies and their impacts on employers, the U.S. Department of Labor contracted with WESTAT, Inc., of Rockville, Maryland, to conduct a thirty-minute telephone

survey with each of more than 5,300 employers. This monograph makes use of data from interviews with 4,832 establishments with one or more employees in December 1979. The respondents are a stratified random sample of establishments that pay unemployment insurance taxes from thirty separate locations around the country. The probability that particular establishments in these areas would be sampled ranged between .006 and 1.0 depended on establishment size and location. This chapter presents tabulations that are weighted to produce estimates of population characteristics for all of our sites combined. The methodology and sampling frame of the EOPP/TJTC employer survey are described in Appendix A.

The chapter is divided into six parts. The first four parts discuss the determinants of a firm's utilization of the three employment subsidy programs we are examining: TJTC, WIN Tax Credits, and CETA-OJT. Separate subsections treat (1.1) knowledge of the programs, (1.2) hiring of eligible workers by those familiar with the program, (1.3) the number of subsidized workers per participating firm, and (1.4) examines overall utilization rates. Section 1.5 examines the retention of subsidized new hires. The final section, 1.6, compares the characteristics of subsidized and unsubsidized new hires.

1.1 Knowledge of Employment Subsidy Programs

Ignorance of the existence or nature of employment and subsidy programs is a very significant barrier to employer participation. At the time of our interviews in spring 1980, only 33 percent of the employment opportunities represented by respondents to our survey were in firms that reported being "familiar" with TJTC. Other government programs were hardly better known: 48 percent of companies (weighted by size) reported being familiar with WIN and 70 percent reported general familiarity with CETA.

Because they have full-time personnel managers, are more likely to be hiring new workers, and are more likely to be approached by government agencies, large establishments can be expected to be more familiar with government programs such as TJTC, WIN, and CETA than the owner-managers of small firms. The data in table 1.1 confirm this expectation. At establishments with 200 or more employees, over half of our respondents were familiar with TJTC; over two-thirds were familiar with WIN, and nearly 90 percent were familiar with CETA-OJT. At establishments with fewer than five employees, only 16 percent of the managers were familiar with TJTC, only 24 percent were familiar with WIN, and 49 percent were familiar with CETA-OJT. The data also confirm the expectation that firms that list job openings with the employment service serving their area were more likely to be familiar with government employment programs: 25 percent of firms with fewer than five employees that listed job openings at the employment service in the preceding year reported being familiar with TJTC; 32 percent, with WIN; and 56 percent, with CETA. Among firms that did not list with the employment service, the figures were 15, 24, and 49 percent, respectively.

To some extent, TJTC's low salience resulted from its newness. Of those familiar with TJTC, 43 percent had first heard of it after September 1979; 29

TABLE 1.1

EMPLOYER FAMILIARITY WITH SELECTED GOVERNMENT PROGRAMS, BY SIZE OF ESTABLISHMENT
SPRING 1980

Program	% Firms Familiar with Programs Who Had						% Total Employment In Sample	% Establishments In Sample
	1-4 empl.	5-19 empl.	20-49 empl.	50-199 empl.	200-500 empl.	500+ empl.		
TJTC								
All employers	16	18	24	34	57	47	33	17
List at Employ. Serv.	25	13	27	39	55	45	40	22
WIN								
All employers	24	28	41	45	69	71	48	29
List at Employ. Serv.	32	34	51	57	72	81	64	40
CETA								
All employers	49	54	64	69	84	91	70	53
List at Employ. Serv.	56	62	71	73	85	94	80	63
NJTC								
All employers	39	47	50	62	74	56	56	45
List at Employ. Serv.	56	49	55	67	73	60	62	54

	Census Survey					
	1-9 empl.	10-49 empl.	50-249 empl.	250-499 empl.	500+ empl.	
NJTC	31	44	53	78	89	—	34

SOURCE: EOPP/TJTC Employer Survey

NOTE: These answers are in response to the question "Are you familiar with _____?" The Census Survey of February 1978 used the question, "Did your company know about the New Jobs Tax Credit before you received this survey form?" Columns 1-7 weight establishment characteristics by employment in that establishment. Column 8 is an unweighted count of establishment characteristics.

percent of those familiar with WIN and 15 percent of those familiar with CETA had also first heard of the programs after September 1979. Familiarity with all these programs, especially TJTC, is rising so it can be hoped that time and aggressive promotion will make employers more aware of the program.

Nevertheless, the low level of familiarity with TJTC sixteen months after passage of the enabling legislation suggests that in many areas of the country

the program was poorly implemented. The contrast with the New Jobs Tax Credit is instructive. In contrast to the extensive advertising campaign for TJTC, no efforts were made to advertise NJTC. Yet a Census Bureau survey done eight months after NJTC was passed found that 31 percent of employers with fewer than ten employees, and 89 percent with more than 500 employees, knew of the program.[1]

1.2 Hiring of Eligible Workers by Employers Familiar with the Subsidy Programs

Only a small proportion of firms that are familiar with employment subsidy programs actually participate in them. Only 13 percent of the firms (representing 23 percent of the employment) familiar with TJTC had hired a worker through that program, and 3 percent of the firms (representing 9 percent of employment) familiar with WIN had hired through the WIN program in 1979. Six percent of the employers (representing 17 percent of employment) reported themselves as being familiar with CETA-OJT had hired a worker referred by CETA in 1979. Only 3 percent of the employers familiar with CETA-OJT (representing 7 percent of employment) actually received subsidies for participating in the CETA-OJT program.

An examination of table 1.2 reveals that there is a strong association between size of an establishment and the likelihood, given familiarity, that it will utilize one of the subsidy programs. Establishments with 200 to 500 employees had a 22 percent likelihood of hiring a TJTC-eligible worker if management was familiar with the program. Only 6 percent of the small firms (one to four employees) that were familiar with TJTC were utilizing it. The comparable contrast between large and small firms was 14 percent versus 2 percent for WIN and 9 percent versus 2 percent for CETA-OJT. Small establishments were less likely to participate primarily because they were hiring fewer new workers. When we limit the sample to establishments that were familiar with a program and that we know hired at least one new worker in 1978 to 1979, the small establishment's probability of hiring a subsidized worker rises to 12 percent for TJTC, 3 percent for WIN, and 4 percent for CETA-OJT.

Another important determinant of an employer's utilization of the subsidy schemes is willingness to accept referrals from the local employment service. Only 17 percent of the small establishments with a recent new hire had listed a job with the employment service the previous year. Those small establishments that did list with the employment service, however, were six times as likely to hire a WIN-eligible worker and four times as likely to contract for a CETA-OJT worker. This is not surprising when one realizes that the employment service is often the CETA subcontractor that arranges CETA-OJT contracts and is the primary mechanism for placing WIN- and TJTC-eligible job seekers. Note that the effect of listing with the employment service was much smaller for TJTC utilization--an increase of 100 percent rather than 400 percent. This is perhaps because the employment service generally receives incentive payments for arranging CETA-OJT contracts and for placing WIN referrals, but not for promoting TJTC vouchering or certification.

TABLE 1.2

PARTICIPATION IN EMPLOYMENT SUBSIDY PROGRAMS IN 1979, AMONG FIRMS FAMILIAR WITH THEM

Program	% Firms Familiar with Programs Who Had						% Total Employment In Pop.	% Establishments In Pop.
	1-4 empl.	5-19 empl.	20-49 empl.	50-199 empl.	200-500 empl.	500+ empl.		
TJTC[a]								
All employers	6	6	33	23	22	25	23	13
List at Emp. Service	6	7	14	23	27	26	24	14
Emp. that hired recently[b]	12	7	36	24	21	25	24	18
WIN[a]								
All employers	2	2	4	9	11	12	9	3
List at Emp. Service	12	5	5	13	12	12	11	7
Emp. that hired recently	3	2	4	9	11	12	9	3
CETA-OJT[c]								
All employers	2	2	3	5	9	16	7	3
List at Emp. Service	4	2	3	7	10	18	11	5
Emp. that hired recently	4	3	3	5	9	16	8	4
All CETA referrals[d]	4	4	13	9	17	38	17	4

SOURCE: EOPP/TJTC Employer Survey

NOTE: Columns 1-7 are tabulations in which establishments were weighted by their employment.

[a]Participation is defined as hiring a worker "through the TJTC program" or "the WIN program." (NAs and don't knows are interpreted as nonparticipant.)

[b]Firms that have "hired recently" are those firms that had new hires in the fourth quarter of 1979 or hired at least one unskilled or semiskilled employee since January 1978.

[c]Participation is defined as receiving "a subsidy for any employee hired through CETA."

[d]Note that many workers referred by CETA do not carry an OJT subsidy with them.

1.3 The Number of Subsidized Workers per Firm

The first wave of the EOPP/TJTC Employer Survey did not obtain data on the number of subsidized workers by each separate program. In most cases, however, particular firms were using only one program. Those using TJTC only reported receiving a tax credit for an average of 3.12 employees or for 3 percent of their employees. Those using WIN only received a tax credit for an average of 3.3 workers or for 4 percent of their employees. CETA-OJT contractors in 1979 got subsidies for an average of 1.91 employees or for 7.0 percent

of their employees. Firms that combined TJTC with other programs were typically larger, and obtained subsidies for a total of 12.7 employees on average or for 5.3 percent of their employees.

The most important determinant of the total number of subsidized employees at a particular establishment is, of course, its size (see table 1.3). The smallest establishments typically received a subsidy or tax credit for only one or two workers. Large establishments (over 500 employees) averaged over thirty-five subsidized employees. In proportion to their size, however, the smallest establishments received the largest benefits. The ratio of subsidized workers to June 1979 employment was 62 percent for subsidized establishments with one to four employees and 3.4 percent for establishments with more than 200 employees. For all programs and all participating firms combined, the ratio of subsidized workers to total employment was approximately .05.

TABLE 1.3

NUMBER OF SUBSIDIZED WORKERS PER SUBSIDIZED FIRM

	Subsidized Firms with						
	1-4 empl.	5-19 empl.	20-49 empl.	50-199 empl.	200-500 empl.	500+ empl.	Full Sample
Number of subsidized workers	1.7	1.5	2.5	4.4	10.2	40.3	3.8
Ratio of subsidized to July 1, 1979 total employment	62%	17%	8.2%	4.9%	3.3%	3.5%	5.0%

SOURCE: EOPP/OJT Employer Survey based on the sampled firms that received a subsidy in 1979.

1.4 Overall Utilization Rates

Only a tiny proportion of the nation's 3.5 million employers receive targeted wage subsidies. Between 1972 and 1975, an average of only 7,100 tax returns claimed WIN tax credits each year. In the three-year period from 1977 to 1979, an average of only 15,000 tax returns or 0.4 percent of all employers claimed WIN tax credits. Our sample produced a very similar estimate of 0.4-0.5 percent WIN participation over the same three-year period.[2] For 1979 a rough estimate of the proportion of firms using these tax credit programs is 0.45 percent for WIN, 0.75 percent for TJTC, and 1.4 percent for CETA-OJT. A comparison of the 1979 and 1977 utilization rates for WIN and CETA-OJT suggests that some progress is being made in expanding utilization of these programs.

The probability of receipt of a wage subsidy rises dramatically with the employer's size (see table 1.4). Among the smallest establishments, 0.5 percent were receiving TJTC tax credits and 0.4 percent WIN credits. Of the

establishments with more than 500 employees, 11 percent were receiving TJTC and 8.9 percent WIN tax credits. The rise in the probability of participation in CETA-OJT is only slightly less dramatic: from 1.1 percent for smallest employers to 14.6 percent for the largest.

TABLE 1.4

RECEIPT OF TARGETED EMPLOYMENT SUBSIDIES
BY ESTABLISHMENT SIZE

	% Firms Receiving Subsidies, by No. of Employees						Proportion of	Proportion
	1-4	5-19	20-49	50-199	200-500	500+	Empl. in Estab.	of Estab.
TJTC	0.53%	0.46	1.3	4.0	5.8	10.9	4.2	0.75%
WIN (1979)	0.41%	0.18	0.9	2.4	3.2	8.7	2.9	0.45%
WIN (1977)	0.29%	0.25	0.5	1.0	2.0	5.0	1.6	0.37%
CETA-OJT (1979)	1.08%	1.2	1.7	3.4	7.9	14.6	5.3	1.5%
CETA-OJT (1977)	0.37%	0.60	0.8	0.7	6.0	0.0	1.2	0.51%
	Number of Subsidized Employees							
Average number of subsidized employees	.046	0.07	0.18	0.56	2.02	9.86		.15
Ratio subsidized/ June 1979 total employees	1.23%	0.31	0.25	0.39	0.42	0.86		0.49%
Ratio subsidized/ total new hires	1.47%	0.48	0.44	0.64	0.89	2.45		0.87%

The number of subsidized employees also rises with the size of the establishment. Row 6 of table 1.4 combines the effects of the probability of participation in 1979 with the amount of subsidy received. Not surprisingly, employers with fewer than five employees averaged only .041 subsidized employee per establishment; employers with more than 500 employees averaged 9.9 subsidized employees--214 times as many. Relative to employment, however, the number of subsidized employees declined with establishment size. The large establishments in our sample typically had 400 times as many employees, however, so relative to their size, they benefited less from the programs than small firms. In row 7 of table 1.4 we present our estimate of the ratio of subsidized to total employment. Because our sample of participating firms is so small, these estimates are quite sensitive to the manner in which the observations are weighted. When sampling weights are used to estimate ratios for the entire study area, the highest ratio, 1.23 percent, is for establishments with zero to four employees and the next highest ratio, 0.86 percent, is for the very largest establishments. When observations are not weighted, the

highest ratio, 1.3 percent, is for establishments with twenty to forty-nine employees and the lowest ratio, 0.3 percent, is for large establishments.

Turnover is considerably smaller in large establishments than in small establishments. Relative to their size, large establishments hire fewer workers and therefore have fewer opportunities to hire a subsidized worker. Row 8 of table 1.4 presents the ratio of subsidized new hires to total new hires. In weighted data there seems to be a tendency for this ratio to rise as the establishment grows from 10 to more than 500 employees. In unweighted data there is no systematic tendency for this ratio to rise or fall with firm size.

In the areas studied the ratio of subsidized employees to total employed was .005 in 1979. If our sampled areas are typical, this implies that the three programs combined subsidized between 300,000 and 400,000 private sector, new hires. For the WIN tax credit and the CETA program, data are available only by fiscal year. In FY 1979, 42,713 people were certified for a WIN tax credit, and 156,000 persons participated in CETA-OJT programs under Titles IIB and IIC. In calendar year 1979, 22,480 WIN-OJT contracts were written and 108,730 TJTC eligibles certified. Thus, the 330,000 total calculated from program data quite closely corresponds to the estimate received from our sample of firms. The average stock of subsidized employees during 1979 would be considerably smaller because CETA-OJT subsidies typically last only six months and the TJTC program was expanding as the year progressed.

The proportion of eligible workers who receive subsidy from these programs is quite small. The best estimate is that the number of TJTC eligibles who obtained new jobs in the first nine months of 1980, when 197,000 were certified, is at least ten times the number of TJTC certifications issued.[3] The statistics for WIN and JOBS, the predecessor of CETA-OJT, are comparable. Hamnermesh (1978) reports that in the JOBS program, contractual arrangements were made for only one-third of the eligible JOBS enrollees who were hired. In fiscal 1979, only 14.4 percent of the non-PSE job placements facilitated by the WIN programs resulted in a certification of the employer's eligibility for the WIN tax credit. Although some of the people eligible for WIN and TJTC tax credits obtained unsubsidized public or nonprofit jobs that were not eligible for a tax credit, and some firms that knew themselves to be eligible chose not to apply, the primary reason that certifications were not requested was employer ignorance: some WIN offices do not inform employers that hire WIN enrollees that they have become eligible for a tax credit.

1.5 The Retention of Subsidized New Hires

The number of subsidized workers newly hired in 1979 that were still working at the firm when the interview was conducted between four and sixteen months later was remarkably high: 46 percent for TJTC, 42 percent for WIN, and 50 percent for CETA-OJT. If we assume the subsidized new hires were hired evenly throughout the year, the implied average monthly separation rates are roughly 7.5 percent for TJTC, 8.3 percent for WIN, and 6.7 percent for CETA. The percentage of <u>all</u> new hires during the fourth quarter of 1979 that were

still with the firm four to seven months later was 50.5 percent. The implied average monthly separation rate, approximately 11.7 percent, is considerably higher than the separation rate of subsidized new hires. Those workers eligible for subsidies typically have higher propensities to quit or be fired, so either the receipt of a subsidy reduces the separation rate or there is a selection process that results in the firm being more likely to file for a subsidy for workers who remain with the firm awhile. For the WIN tax credit, such a selection process may arise from a provision that limits eligibility to employees who remain with a firm for at least thirty days. CETA-OJT contracts often contain a promise "to retain all on-the-job training employees upon completion of their training" (so states the standard OJT contract for Mobile, Alabama). Even where monitoring and enforcement of this provision are spotty, the language tends to screen out employers who are not offering long-term jobs and may also create a sense of moral obligation to retain OJT-subsidized workers. A selection process that produces a high retention rate of subsidized employees also will result if firms choose not to apply when the subsidy is small or if firms do not learn an individual is eligible until many months after he or she has started work.

Opponents of wage subsidies for hiring the disadvantaged sometimes argue that they only create "dead-end" jobs and therefore do not benefit the subsidized workers. While the second proposition is in no way implied by the first (even a bad job may be preferred by those who find unemployment demoralizing and who may well be able to use the job as a stepping stone to something better), it would, nevertheless, be interesting to see whether the first proposition is true. Those who believe it cite "high" rates of turnover in the firms that use the program as evidence. Our survey demonstrates, however, that the rates of turnover are no higher in firms using wage subsidies than in the other firms in our sample. (It also demonstrates that subsidized workers have lower rates of turnover than other workers, but this, however, could be a result of firms not discovering worker eligibility until long after hiring them.)

The aggregate quarterly separation rates for the fourth quarter of 1979 were 11.7 percent for the full sample and 10.7 percent for firms that used one of the subsidy programs. In both samples, a little more than 50 percent of new hires in the fourth quarter were still at the firm at the time of the interview, despite the fact that firms with high turnover are hiring more frequently and thus are more likely to hire an eligible worker, and despite the fact that, relative to their employment, small establishments, which traditionally have higher rates of turnover, are heavier users of the schemes than large establishments (see table 1.4).

Another way to explore this issue is to examine the variation in industry specific ratios of subsidized to total employment (see row 1 of table 1.5). The industries with the lowest usage of the wage subsidy schemes are finance and eating and drinking establishments. Eating and drinking establishments are archetypal residents of the secondary labor market. Restaurants and bars pay hourly wages that are considerably lower than the minimum because tips provide much of their staff's compensation. Tips are not eligible for subsidy so these firms have only minimal incentives to participate, and utilization

TABLE 1.5

RECEIPT OF WAGE SUBSIDIES AND RETENTION OF SUBSIDIZED NEW HIRES, BY INDUSTRY
(IN PERCENTAGES)

	Const.	Mining, Manuf. & Util.	Whole-saling	Retail	Eating & Drinking	Finance	Personal & Repair Services	Prof. Serv.	Full Sample	No. of Firms
Ratio of subsidized/ total employment	0.25	0.67	0.61	0.48	0.12	0.37	0.32	0.64	0.49	
Ratio of subsidized/ total new hires	.3	2.0	1.9	0.7	0.1	1.1	.4	1.5	0.87	
Retention ratios										
New hires at all firms[a]	36	56	62	49	39	74	52	66	50.5	4412
All new hires at firms using subsidy[b]	32	59	62	41	54	76	54	56	53.7	273
Workers subsidized by										
TJTC[b]	9	43	60	52	71	69	55	71	46	76
WIN[b]	50	35	17	45	68	27	34	85	42	75
CETA-OJT[b]	25	54	50	46	89	24	33	65	50	75

SOURCE: EOPP/TJTC Employer Survey

[a]New hires during the fourth quarter of 1979, still with the firm at the time of the interview, between March and May 1980.

[b]Subsidized new hires during 1979, still with the firm at the time of the interview. Sample limited to firms that had first heard of the program prior to September 1979. Firms that participated in more than one program appear in more than one row. Firms that hired large numbers of subsidized workers have a heavy weight because ratios are based on aggregates.

rates are very low (0.1 percent of employment). The heavy users of the wage subsidy schemes are mining, manufacturing, and utilities (0.67 percent), wholesaling (0.61 percent), and professional services (0.64 percent). There seems to be no association, within industries, between high turnover rates and use of subsidy programs. By comparing rows 3 and 4 of table 1.5, we can see that, in general, the firms that participate in subsidy programs seem to have retention ratios similar to those of other firms in the industry. The only exception is eating and drinking places, where the subsidy recipients seem to have higher than average retention ratios.

1.6 Characteristics of Subsidized and Unsubsidized New Hires

The purpose of targeted employment subsidy programs is to induce firms (1) to hire disadvantaged workers for jobs that would otherwise be filled by better qualified workers and (2) to provide the extra training these workers require so that they may eventually reach the productivity standard of the other workers in the firm. In this subsection we analyze a preliminary tabulation of the characteristics of the most recently hired subsidized and nonsubsidized workers, to determine whether the subsidy schemes are, as intended, benefiting workers with poorer than average qualifications, and whether additional training has been provided them.

The first four rows of table 1.6 present characteristics of the most recently hired person. As anticipated, subsidized new hires are generally younger, have less schooling, and have half as much useful experience as unsubsidized new hires. A comparison of the first and sixth columns of the table reveals that unsubsidized new hires at firms that use subsidy programs are no different from unsubsidized new hires at firms that do not use these programs. Thus, the difference between the qualifications of subsidized and unsubsidized new hires is not a consequence of employers who have less demanding requirements using the programs more intensively. The difference results from subsidized firms lowering the qualifications required of subsidized new hires and/or assigning them to the least demanding of their job openings. This latter interpretation is supported by the fact that in the study, subsidized new hires typically started at a lower wage rate (8 percent lower). Opportunities for increases in wage rates were similar: for both types of workers the top wage rate in that job averaged 30 percent more than the starting wage. Unsubsidized workers seem to do better, however. Firms that received a subsidy had retained 77 percent of their unsubsidized new hires and had given wage increases of 10 percent over and above improvements in scale wage rates. Only 50 percent of the subsidized new hires were still working at the firm; they had experienced an average wage increase of 3 percent over and above improvements in the scale wage. This comparison does not, however, control for the length of time the worker has been with the firm, so firm conclusions must wait until more complete models are estimated.

The hiring process for subsidized workers seems to be distinctly different from the hiring process of the typical unsubsidized new worker. Firms

TABLE 1.6

CHARACTERISTICS OF THE MOST RECENT SUBSIDIZED AND UNSUBSIDIZED NEWLY HIRED WORKER

Worker and Job Characteristics	All Nonsubsidized Workers	Subsidized Worker			Workers in Firms Hiring Both	
		TJTC	WIN	CETA	Subsidized	Unsubsidized
Number of Cases						
Characteristics of the new hire						
% Male	49	44	35	64	52	46
Useful experience (months)	44.7	23.3	17.2	20.7	20.6	47.6
Age (years)	27.2	23.6	26.6	24.2	24.2	26.8
Schooling index[a]	4.15	3.6	3.5	3.8	3.7	4.2
The hiring decision						
Days vacancy open[b]	38	67	33	8	26	43
Number interviewed[c]	4.5	4.4	4.0	4.7	4.1	5.4
Number referred by ES, etc.[c]	.5	1.4	1.4	2.3	1.8	1.0
Staff time selecting new hire (hours)[c]	6.1	5.6	5.2	7.0	5.7	11.3
% Turning down job offer	20	4	9	12	7	11
Characteristics of the job						
Starting wage rate ($)	4.15	3.55	4.22	3.84	3.87	4.21
Top wage rate ($)	5.43	4.27	4.98	5.23	5.05	5.45
Current wage of those still with firm ($)	4.58	3.62	4.19	4.01	3.98	4.65
Experience with the worker						
% Still at firm	70	75	53	38	50	77
Training time by other employees (hours)	13	15	15	19	18	13
Training time of personnel & supervisors (hours)	19	14	12	23	20	18
Productivity index (second week)[d]	56	48	48	46	44	54
Productivity index (most recent)[d]	71	69	63	62	61	71

[a]Schooling index is coded: some high school = 3; high school graduate = 4; some college = 5; and college graduate = 7.

[b]Based on the question "How long was it between the time you started to recruit for the job and the time _____ started work?" The 28 percent of firms that said they did not recruit were coded zero, and the 2.5 percent that said they are always looking were coded 996 days.

[c]For firms that did recruit.

[d]Index runs from zero to 100.

that have hired a subsidized worker are distinctly more likely to be evaluating referrals from government agencies even when a nonsubsidized worker is hired. And for openings that end up being filled by a subsidized new hire, the use of government referrals is even greater (1.8 interviews of government referrals compared to only 1 for the typical nonsubsidized new hire). Nevertheless, less than half of the subsidized workers who were hired were referred by a government agency. This was true even for CETA-OJT hires. This suggests that either some firms go to CETA after the fact to certify as eligible for OJT subsidy a worker they have already hired or that when they have arranged an OJT contract most firms keep their options open (by evaluating walk-ins as well as CETA referrals) in case the workers referred by CETA turn out to be unacceptable. Unfortunately, we did not ask whether the person hired was a referral from a government agency, so we cannot distinguish between these two possibilities.

Firms that use subsidy programs seem to invest more in the hiring decision. When they hire an unsubsidized worker, they interview an average of 5.4 job seekers and they invest 11.3 hours of staff time in the decision, compared to interviews with 4.5 job seekers and 6.1 hours spent for firms not using subsidy programs. When they hire a subsidized worker, however, they invest only 5.7 hours of staff time in the decision, and they interview an average of only 4.1 applicants. Later research will attempt to determine whether this differential reflects the types of jobs the subsidized new hires are being placed in, or whether it is an impact of subsidy availability or the screening service provided by the government agencies referring workers.

The final difference between subsidized and unsubsidized new hires is their productivity. As anticipated, subsidized new hires are less productive than unsubsidized new hires. The difference is not very great, however: 19 percent in the second week of employment and 14 percent at the time of interview. Since, however, the subsidy is equal to 50 percent of the wage costs and probably 40 percent of total compensation, the employer is more than compensated for the reduced productivity of the subsidized workers. The time spent by other employees and staff orienting and training the new employee during his/her first month on the job seems to be slightly higher for subsidized workers. The program that seems to result in the greatest investment of staff time in training the new hire is CETA-OJT. Both the total number of hours--forty-two hours for CETA-OJT versus twenty-nine hours for TJTC and thirty-one hours for nonsubsidized workers--and differences between the different programs seem quite small, however.

NOTES

1. The finding that only 30 percent of small employers knew about NJTC was interpreted by some at the time as evidence that NJTC was not effective in expanding employment. This judgment was certainly premature, however, for the NFIB surveys were indicating that over the six-month period, January to July 1978, the percentage that knew about NJTC rose by 15 percentage points, and program data now indicate that in its second and final year more than 28 percent of all employers in the nation received a NJTC credit. Even if most small employers had remained in ignorance, larger firms that account for most of the nation's jobs, were from the beginning much more aware of the credit. While 34 percent of all employers were aware of NJTC in February 1978, the employers that did know about the credit were responsible for roughly 60 percent of all private sector jobs.

2. The closeness of the survey's estimate of the WIN participation rate and total numbers of subsidized employers to program data suggests that the nonrandom selection of geographic areas for study did not significantly bias our sample of employers.

3. There are 7-9 million newly hired workers every month and 55 percent of these are under age 25 (Cohen and Schwartz, 1979). Certainly at least 10 percent of the age group is eligible, so the average monthly certification rate of 22,000 from October 1, 1979 to September 30, 1980 implies a participation rate of 6 percent or less.

APPENDIX 1A

A Brief Description of the First Wave of the Employer Survey

WESTAT, Inc. of Rockville, Maryland was the survey contractor. They obtained completed interviews with 5,859 employers. Of these, about 486 were with private employers who had a CETA-OJT contract during 1978 or 1979, 33 with taxi companies and 5,340 with employers selected randomly from ES202 or DMI lists. Interview time ranged from less than twenty minutes for firms with very few employees to two hours or more for firms with multiple establishments and several hundred employees. A screener and a main questionnaire were used for all interviews. If the employer requested more information on the survey, a questionnaire explanation and work sheet were mailed to the employer. The interview was then conducted over the telephone after receipt of the materials. For large and medium sized firms, there were normally two or three respondents per firm. Small firms generally had one respondent.

Table 1.7 lists the sites and response rates obtained in each site. Overall, refusal rates were very low for this type of study. However, the sites located in Ohio and Louisiana stand out as exceptions to the rule. The refusal rates for these sites range from 2 percent to over 11 percent above the average for all sites. Also, the number of max-call cases is somewhat higher in these sites. We suspect that some of these cases may have been "avoidance" cases--cases in which the respondent had no intention of completing the interview but felt that if she/he put the interview off long enough, the interviewer would stop calling and she/he would not be forced to actually refuse.

Sample Design of the Employer Survey

The probability sample. The primary sample frames for the Employers Survey consisted of lists of business units that, in compliance with the requirements of State Unemployment Insurance laws, file quarterly reports on employment with state employment security agencies--the ES202 lists. These reports were expected to provide a virtual census of the workers of private nonagricultural employers, and are the benchmark upon which National Income Account estimates of employment and compensation are based. Since the law requires that newly formed businesses file for an employer identification number before the end of the quarter in which they hire their first employee, the lists were expected to be quite up-to-date. The ES202 listings of employers contain the 4-digit SIC code and a count of the number of employees in the first quarter of 1979 for each reporting unit.

State laws regarding the confidentiality of the ES202 list in Kentucky, Alabama, and Ohio necessitated using alternative sampling frames in these states--the Dun and Bradstreet Market Identifier Files (DMI). While not being quite as comprehensive nor as up-to-date as the ES202 list, the DMI does provide the information necessary to replicate the sample selection procedures

TABLE 1.7

Site[1]	Number Completes	Completion[2] Rate	Refusal[3] Rate	Response[4] Rate
Alabama				
Mobile	358	58.7	21.1	75.4
Birmingham	220	56.8	20.0	73.3
Pensacola, FL	142	52.8	19.8	75.5
Kentucky				
Pike	232	59.2	11.1	86.6
Buchanan/Dickenson, VA	121	56.3	9.0	89.0
Harlan	103	61.3	7.2	86.5
Louisiana				
Baton Rouge	337	48.1	26.7	67.8
Beaumont/Port Arthur, TX	178	49.2	21.6	72.3
Lake Charles/Lafayette	157	55.9	20.3	75.8
Missouri				
Central Missouri	279	58.7	13.3	83.5
Southeast Missouri	150	59.8	9.6	87.7
Northwest Missouri	132	66.3	10.8	88.0
Ohio				
Columbus	420	52.9	25.1	69.4
Toledo	205	55.7	25.2	70.7
Cincinnati	235	49.3	26.1	67.3
Texas				
Corpus Christi	343	52.4	20.2	73.8
San Antonio	227	51.8	19.8	73.0
New Orleans, LA	176	39.7	29.6	63.1
Washington				
Southwest Washington	294	54.8	1.20	82.8
Skagit/Watcom	155	63.5	12.4	83.8
Olympia Peninsula	114	49.1	23.5	73.1
Colorado[5]				
Weld	112	36.0	1.8	97.4
Alamosa	58	37.9	--	100.0
Logan/El Paso	60	36.1	6.2	93.7
Wisconsin[5]				
Marathon	142	45.9	4.0	95.9
Outagamie	61	31.8	4.7	95.3
Winnebago	57	33.1	8.1	91.9
TOTALS	5,068	51.7	18.5	76.5

[1]Under heading, site listed first is Pilot; site listed second is Household Control; site listed third is Employer Control.

[2]Completion Rate = (# of Completes + # Partial Completes) Total # of Finalizations.

[3]Refusal Rate = # of Refusals (# of Complete + # of Partial Completes + # of Refusals).

[4]Response Rate = (# of Completes + # of Partial Completes) ÷ (# of Completes + # of Partial Completes + # of Refusals + (Max-Calls x 67%)).

[5]For budgetary reasons these regions were eliminated from the sample midway through the interviewing period.

based on employment and SIC code planned for the ES202 frame and, therefore, fills the gaps in our ES202 listings quite well.

The industrial universe which the Employers Survey represented included all nonagricultural for-profit employers that have unemployment insurance accounts. Agriculture, forestry, and fisheries (SIC Code 00-09) were excluded because of the poor coverage of these industries in the ES202 files. Also, excluded were government and government enterprises (SIC Codes 43, 90-99) and non-profit organizations (SIC Codes 821, 822, 823, 84, and 86). Since government and non-profit organizations are not limited to the above SIC codes, an initial screening determined whether the organization contacted was non-profit or governmental, and the interview was terminated if it was. The ES202 and DMI lists of employers were also checked against other employer lists--membership lists of the local Chamber of Commerce, lists of local manufacturers--and with the local CETA prime sponsor to insure that no really large local employers were inadvertently left out of the sample frame.

The supplementary sample of employers with CETA OJT contracts. Only a tiny proportion of the employers in a labor market negotiate and sign OJT contracts with CETA. Consequently, a random sample of 6,000 employers was expected to yield only about 200 employers that had OJT contracts with CETA. An analysis of employers' decisions requires many more observations than that. Therefore, a supplementary sample of approximately 490 employers that had CETA OJT contracts in 1978 or 1979 was drawn to provide additional observations on this class of employers. The program records of the CETA prime sponsors in pilot and control sites was the source of the list of OJT contractors from which this sample was drawn.

Geographic coverage of the employer survey. The employer survey was conducted in twenty-eight sites dispersed around the nation. Ten of the sites were selected because the Department of Labor was running a major social experiment, the Employment Opportunity Pilot Projects (EOPP), in these labor markets. Eighteen other locations were selected to form a control group for planned studies of the impact of EOPP. Both rural and urban, Northern and Southern employers are represented. While the sites were not randomly selected, the local economies that were included seem to be representative of the nation. They range from an Appalachian coal community to a Pacific Northwest logging area from a Midwestern industrial center, Columbus, to Corpus Christi, a center of the oil and petrochemical industries. Table 1.8 lists the counties that were included in each site and total private non-agricultural employment of each site.

Selection of the sample. Stratified random samples of UI tax filing units were drawn from the ES202 lists. Where the ES202 lists were unavailable (i.e., Kentucky, Alabama, and Ohio), stratified random samples of establishments were drawn from the Dun and Bradstreet Market Identifier File. The sampling procedure for selecting the employers involved the following steps:

1. A sampling measure of size was assigned to each employer in the frame based upon the estimated number of low-wage workers. These measures of size, Z_j were computed from the formula:

TABLE 1.8

GEOGRAPHIC COVERAGE OF EMPLOYER SURVEY

Site	Pilot/ Control	Total Private Employment In Site	Counties
Alabama			
Mobile	P	115,738	Baldwin, Escambia, Mobile Co.
Birmingham	C	271,202	Jefferson, Shelby, Walker Co.
Pensacola	C	77,684	Escambia, Okaloosa, Santa Rosa Co.
Colorado			
Weld County	P	25,207	Weld County
Alamosa County	C	20,000	Alamosa County
Logan, El Paso County	C	37,348	Logan, El Paso Co.
Kentucky			
Pike County	P	15,645	Pike County
Buchanan, Dickenson Co.	C	14,861	Buchanan, Dickenson Co.
Harlan County	C	8,382	Harlan County
Louisiana			
Baton Rouge	P	104,299	East Baton Rouge Parish
Beaumont-Port Arthur	C	114,064	Hardin, Jefferson, Orange Co.
Lake Charles	C	87,457	Calcasieu Parish, Lafayette Parish
Missouri			
Central Missouri	P	30,067	Carroll, Charlton, Johnson, Lafayette, Pettis, Saline Co.
Southeast Missouri	C	38,165	Bollinger, Cape Girardeau, Iron, Perry, St. Francois, Ste. Genevieve Co.
Northwest Missouri	C	39,847	Buchanan, Caldwell, Clinton, Daviess, Grundy, Livingston Co.
Ohio			
Columbus	P	303,325	Franklin County
Cincinnati	P	402,091	Hamilton County
Toledo	C	171,451	Lucas County
Dayton	C	250,000	Montgomery County
Texas			
Corpus Christi	P	103,532	Aransas, Bee, Brooks, Duval, Jim Wells, Kenady, Kleberg, Live Oak, McMullen, Nueces, San Patricio Co.
San Antonio	C	288,855	Bexar, Comal, DeWitt, Gonzalez, Guadalupe, Karnes, Victoria, Wilson Co.
New Orleans	C	211,892	Orleans Parish
Washington			
Southwest Washington	P	43,216	Cowlitz, Grays Harbor, Pacific, Wahkiakum Co.
Skagit, Whatcom County	C	36,959	Skagit, Whatcom Co.
Olympia Peninsula	C	20,453	Jefferson, Lewis, Mason, Skamania Co.
Wisconsin			
Marathon County	P	30,978	Marathon County
Outagamie County	C	43,113	Outagamie County
Winnebago County	C	45,313	Winnebago County

$$Z_j = [w_i\ (1 + \text{Employment}_j)]^{0.8}$$

where w_i is an estimate of the proportion of "low-wage" employees in the "i"th industry based upon tabulations of the 1970 Census Public Use Tapes for the ten initially defined pilot sites. In order to assure enough observations for a study of the impact of EOPP on out-contracting to low-wage employers, the Z_j for four industries were tripled (SIC 7349, 7362, 7393, 5963).

2. Multi-unit employers within the same site that had the same identification (account) number were consolidated into a single record which was then assigned the measure of size.

3. The certainty class, employers for which $P_j \geq 1$, was determined in accordance with the assumption that the dropout rate in this class would be approximately one-half. (The errors of this assumption will have little effect. They will shift only a few employers, which in any case would have large probabilities of selection, into or out of the certainty class.)

4. The noncertainty sample was selected by arranging the balance of the frame in order of size, assigning all employers that reported zero employment to a single stratum, dividing the remaining employers in the array into six strata each having about the same aggregate size, and choosing, with equal probability, about four times the desired number of completed interviews. The order of the selected establishments has then randomized across all strata.

   In conducting the canvass, the selected employers which were out of business or which were inaccessible because of bad addresses, were deleted by an advance screening operation. Interviews were then attempted for all the remaining certainty employers. For the noncertainty sample, however, interviews were attempted for the first n_h employers in the randomly sorted list where n_h is the desired number of completed interviews for the site.

5. Since the units listed in ES202 were not expected to always correspond to single-location establishments, all selected units were asked whether they operated at more than one location within the target area. Those which did, were requested to submit a single report covering all their locations in the site, if feasible. However, where only separate reports would be obtained, a subsample of establishments was selected and the sampling weights adjusted accordingly to reflect the correct probabilities of selection.

CHAPTER 2

FIRM FAMILIARITY WITH EMPLOYMENT SUBSIDIES

John H. Bishop and Mark Montgomery

2.1 Introduction

It was shown in the last chapter that the effectiveness of employment subsidies in creating jobs is severely hampered by simple ignorance of the programs. Only 17 percent of all employers were familiar with TJTC, and only 23 percent were familiar with WIN. Slightly more than half of the firms in the EOPP Survey were familiar with the CETA-OJT programs, and slightly less than half were familiar with the New Jobs Tax Credit. Firms that are unfamiliar with a program are prevented from responding to its incentives. Therefore, the success of a program depends as much upon employer familiarity with it as it does on their behavior, given that knowledge.

2.2 Determinants of Familiarity with Subsidy Programs

Knowledge of the existence of employment subsidy programs is not distributed randomly across firms. A firm's likelihood of being aware of a given subsidy program will depend upon its characteristics and on the characteristics of the community in which it is located.

Size is likely to be the single most important predictor of an employer's familiarity with subsidy programs. The potential payoff to knowledge about subsidy programs is likely to be greater for a large organization, so the top executive either spends more time learning about such programs or assigns the job to specialized personnel such as the controller, accountant, or personnel officer. In their study of the NJTC, Perloff and Wachter found size to be the only significant variable in the equation estimating the likelihood of familiarity with the program (Perloff and Wachter, 1980). If eligible job applicants know about the program and advertise their eligibility when they contact potential employers, large employers will be more likely to encounter such workers and thus become informed about the program. Most of the agencies responsible for administering these programs, however, are not encouraging eligible job seekers to look for jobs on their own by telling prospective employers about their eligibility for a subsidy. In the TJTC case, most employment service offices are not even informing the eligibles that come to them asking for job search assistance of the program's existence. Consequently, only 1.3 percent of employers in the sample first heard of WIN from a job applicant, and only 3.5 percent first heard of CETA-OJT from a job applicant. As hypothesized, however, large employers are more likely to hear of the program from a job applicant than small employers. For employers with over 200 employees, job applicants are the initial source of information about CETA 7.3 percent of the time and about WIN 3.2 percent of the time. For firms with

fewer than 20 employees only 2.2 percent had heard of CETA-OJT from a job applicant, and only .75 percent had heard of WIN from that source.

One would also expect the agencies responsible for administering these programs to concentrate their promotional efforts on larger establishments. Since large establishments are more likely to be hiring, there is a greater likelihood of being able to place a client through a personal contact with a large employer. Here again our hypothesis was borne out by the data. Of the employers with more than 200 employees, 39 percent had been personally contacted about CETA, and 32 percent had been personally contacted about WIN. Of the firms with fewer than twenty employees, only 14 percent had been personally contacted about CETA, and 5.5 percent had been contacted about WIN.

For many jurisdictions, the employment service is responsible for administering all three of the programs: TJTC, WIN, and CETA-OJT. Therefore, we expect that firms that regularly list their job openings at the employment service office are more likely to be told of the program by ES staff.

We expect growing firms to be more aware of these programs for three reasons. They are more likely to initiate a contact with the ES office. Secondly, they are more likely to come into contact with a job seeker who knows about and makes the firm aware of the program. Finally, the payoff to participating in recruitment subsidy programs and therefore the payoff to investing in knowledge about such programs is greater for the growing firm.

It is important to test for the effect of growth on knowledge for in chapter 4 we will be examining the impact of participation in these subsidy programs on the firm's growth rate. If we can reject the hypothesis that knowledge is caused by the firm's growth rate, we will be able to get unbiased estimates of the impact of participation on employment by using knowledge as an instrument for participation.

The most important geographic determinant of rates of familiarity is likely to be the enthusiasm and effectiveness of the local employment service office's promotion of the program. A study of TJTC implementation in five states (Pritchard 1980) shows that choice of promotional techniques is largely the province of the state and locality, and will vary with the firm's location. The thinness and dispersion of rural labor markets make employer-provided training more effective than classroom training in rural areas. CETA administrators in rural areas have reacted to this fact by concentrating their dollars and marketing efforts on the CETA-OJT program so we anticipate that rural employers will be more familiar with the program. For the other programs, TJTC, WIN, and NJTC, we have no prior hypothesis regarding the sign of the relationship between familiarity and labor market size. For the targeted programs, we expect that local promotional efforts and, therefore, the knowledgeability of local employers will be positively related to the number of eligible individuals.

2.3 Results

Table 2.1 presents the definitions, means, and standard deviations of the variables used in the analysis. Table 2.2 presents the results of the estimation of probit models of the likelihood of familiarity with TJTC, WIN, CETA-OJT, and NJTC. Estimation was conducted on a sample of 4,528 firms that answered all of the relevant questions. (See Appendix B to chapter 3 for details of probit methodology.) The overall (unweighted) familiarity rates were .20 for TJTC, .31 for WIN, .60 for CETA-OJT, and .48 for NJTC.

In a probit model, the derivative of the probability of the event in question (in our case, familiarity) with respect to an independent variable is equal to the coefficient of that variable multiplied by the derivative of F(XB), the standard normal distribution function evaluated at XB. In order to interpret the coefficients in the probit equations, we can use the approximation of F'(X'B), evaluated at the overall familiarity rate, as a multiplier.[5] This will give us a rough idea of the magnitude of the effects of changes in the value of independent variables upon the likelihood of familiarity. The multiplier for calculating the effect of a variable on the probability of familiarity with each program is presented at the bottom of table 2.2.

2.3.1 The Effect of Employer Characteristics

As hypothesized, the single most powerful determinant of familiarity with these programs was the size of the establishment. The effect of size on the probability of familiarity can be determined by calculating the product of the probability multipliers, the coefficient on employment, and the hypothesized change in the log of employment. Employers with ten times as many employees have a .12 higher probability of being familiar with TJTC [.12 = .28 · .188 · 2.3]. The effect of this size differential on the probability of familiarity is .15 for WIN, .12 for CETA-OJT, and .13 for NJTC.

Employers who belong to local business organizations are significantly more likely to be "familiar" with these programs. The effect of such membership on the probability of familiarity is .05 for TJTC, .075 for WIN, .074 for CETA-OJT, and .10 for NJTC. These effects reflect the greater connectedness of these employers to sources of information about subsidy programs. Truly comprehensive lists of local employers are not easy to obtain, so the membership lists of these organizations are convenient targets for outreach efforts. In some communities the administrators of subsidy programs give speeches about the program at meetings of local employer organizations or contract with them to promote the program.

Unionization had no large or consistent effect on the firms familiarity with subsidy programs. The proportion of the establishment's work force that is blue collar (i.e., craftworkers, operatives, laborers, and service workers) has a consistently significant negative effect on the firm's likelihood of familiarity. When combined with the industry dummies, the negative coefficient

TABLE 2.1

DEFINITION OF VARIABLES USED IN PROBIT MODELS OF CHAPTER 2 AND 3

Name	Mean	Standard Deviation	Definition
Employer Characteristics			
Employment Growth 1979	.014	.29	The difference between total hours worked in December 1979 and in July 1979 divided by total hours worked in July.
Proportion Part-Time	.132	.245	The ratio of hours worked by part-time workers to total hours worked.
Proportion Low Skill	.638	.309	The proportion of total hours worked by operatives, laborers, service, sales, clerical, and office workers.
Unionized Establishment	.144	.352	Takes the value of 1 if more than 50 percent of the establishment's work force is covered by collective bargaining agreements.
Proportion Blue Collar	.534	.366	The proportion of total hours worked by craft workers, service workers, laborers, and operatives during the reference pay period.
Log Establishment Size	2.59	1.61	The natural log of number of full-time equivalent employees at the establishment in July 1979.
Size Greater Than 250	.049	.208	Takes the value 1 if establishment had more than 250 full-time equivalent employees in December 1979.
Membership in Business Organization	.488	.494	Takes the value of 1 if the employer is a member of a local business organization; 0 otherwise.
Firing Flexibility	.269	.444	Takes the value of 1 if during the last quarter of 1979 the establishment laid off, fired, or "induced to quit" a worker who would have been retained had he or she been "doing a better job"; 0 otherwise.
40 Hours Full-Time	.687	.463	Takes the value of 1 if the average hours worked by full-time workers are greater than 37 and less than 43 hours; 0 otherwise.
Reliable Unskilled Workers Unavailable	.494	.50	Takes the value 1 if the respondent reports it is "very" or "somewhat" difficult to obtain "reliable unskilled workers at reasonable wages"; 0 otherwise.
Labor Market Characteristics			
Unemployment Rate 1979	6.29	1.53	An average of March 1979 unemployment rates (weighted by employment) of the counties in the site. From American Statistical Index; Employment and Unemployment Statistics by CETA area.
Unemployment Rate 1968	6.09	1.43	A total-employment-weighted average of unemployment rates of the counties at the site (or relevant SMSA) in June 1968.
Proportion in Poverty	.144	.070	A population-weighted average of the proportion of families below the poverty line in the counties at site in 1969. From the 1970 Census of Population.
Log Labor Market Size	11.23	1.35	The natural log of the number of workers in the labor market at the site in 1979. From the American Statistical Index; CETA Area Employment and Unemployment Statistics 1979.
Change in Industry Employment at the Site 1977-78	12.4	.40	The proportional change in employment at the 3-digit industry level from March 1977 to March 1978 for the county at the site having the largest overall employment. (At the majority of sites one county had more than 50 percent of total employment.) In cases where confidentiality prevented reporting of the 3-digit value, the 2-digit value was unavailable, the value for the industry category (e.g., Manufacturing) was used. From County Business Patterns 1977, 1978.
Change in 1-digit Industry Employment at Site	.003	.036	Takes the value of Industry Employment Change if that variable is based on industrial category.
Regional Dummies			The mean of Northeast (Washington State) is .078, Midwest (Missouri) is .144, West (Colorado) is .038, Southeast is .344, and Southwest (Texas) is .099.
Outreach Variables and Previous Usage			
Learned of WIN from a Government Representative	.086	.248	Takes the value 1 if the establishment learned of WIN from a government representative.
Proportion Learned of WIN from Government Representative	.06	.023	The proportion of the other establishments at the site who learned about WIN from a government representative.
Learned of CETA-OJT from Government Representative	.130	.336	Takes the value of 1 if establishments learned of CETA from a government representative.
Proportion Learned of CETA-OJT from Government Representative	.117	.032	The proportion of other establishments at the site who learned about CETA-OJT from a government representative.
Received NJTC	.138	.345	Takes the value 1 if respondent reports the firm received an NJTC credit; 0 otherwise.
Responded to NJTC but Did Not Receive	.012	.108	Takes the value 1 if respondent reports the firm increased employment in response to NJTC but did not receive a credit; 0 otherwise.
Received WIN 1977-78	.025	.157	Takes the value 1 if the establishment received a WIN tax credit in 1977 or 1978; 0 otherwise.
Received CETA-OJT 1977-78	.030	.171	Takes the value 1 if the establishment received a CETA-OJT contract in 1977 or 1978; 0 otherwise.
Industry Dummies			Means are .076 for Construction, .428 for Wholesale Retail, .395 for Finance and Services, and .038 for Transportation and Communication. The excluded industry is Mining and Manufacturing.

TABLE 2.2

FAMILIARITY WITH TJTC, WIN, CETA, AND NJTC
(Probit Models)

	TJTC	WIN	CETA	NJTC
Employer Characteristics				
Employee Growth Rate 1979	.06 (1.22)	.18 (2.58)	.08 (1.11)	
Change in Industry Employment 1977-78		.10 (1.78)	.01 (1.42)	.04 (.69)
Dummy: 2-Digit Sic Industry		-.42 (2.20)	.04 (.38)	.03 (.32)
Dummy: Industrial Category		-.68 (1.23)	.03 (.05)	-.03 (.05)
Log Establishment Size	.19 (11.60)	.19 (12.74)	.16 (10.63)	.15 (11.00)
Unionized	- .06 (.80)	.04 (.70)	.06 (.89)	-.02 (.31)
Proportion Blue Collar	- .20 (3.29)	-.15 (2.56)	-.19 (3.25)	-.19 (3.48)
Membership Business Org.	.17 (3.72)	.21 (4.94)	.19 (4.52)	.26 (6.56)
Market Characteristics				
Unemployment Rate 1978		.02 (1.03)	.04 (2.26)	.00 (.10)
Unemployment Rate 1979	- .03 (1.24)	-.03 (1.17)	.01 (.43)	
Proportion Minority	- .30 (.89)	.13 (.42)	2.02 (6.72)	.33 (1.21)
Proportion in Poverty	.44 (.53)	.19 (.30)	.14 (.22)	-.96 (1.70)
Log Labor Market Size	.07 (1.83)	-.07 (2.00)	-.20 (5.62)	-.08 (2.79)
Northwest	- .27 (2.09)	-.07 (.61)	-.03 (.23)	-.02 (.25)
Midwest	- .18 (1.73)	-.14 (1.40)	-.31 (3.13)	.06 (.74)
Southeast	.08 (.59)	-.21 (1.71)	-.66 (5.41)	-.04 (.36)
Southwest	.32 (2.24)	-.36 (2.75)	-.53 (4.29)	-.09 (.96)
West	.08 (.50)	-.13 (.97)	-.37 (2.82)	.07 (.64)
Past Experience and Outreach				
Received NJTC	.70 (12.13)	.40 (7.00)	.27 (4.39)	
Prop. in Site Who Learned of WIN from Government Rep.	3.1 (2.95)	2.4 (2.01)	2.93 (3.18)	
Industry Dummies				
Construction	- .21 (3.82)	-.34 (3.54)	-.16 (1.70)	.07 (.73)
Transportation & Communication	- .09 (.69)	-.16 (1.39)	-.21 (1.80)	-.10 (.88)
Finance and Services	- .07 (.86)	-.10 (1.37)	.14 (1.88)	-.09 (1.31)
Wholesale and Retail Trade	- .26 (3.53)	-.15 (1.39)	0.17 (2.43)	-.14 (2.17)
Constant	-2.13 (3.82)	-.16 (.30)	1.44 (2.57)	.68 (1.72)
2 x ln Likelihood	456.24	457.79	440.49	270.08
Proportion Familiar	.20	.31	.57	.48
Multiplier for Probability	.28	.35	.39	.40
Sample Size	4528	4528	4252	4528

Asymptotic t statistics are in parentheses.

on percent blue collar implies that construction, transportation, and communication firms are especially ignorant of these programs. Employers in the finance and services industries seem to have no special familiarity with the tax credit programs but have a particularly high level of familiarity with CETA-OJT.

Two measures of the growth rate of employment--an establishment growth rate between July and December 1979 and the local industry growth rate between 1977 and 1978--are included in the model. All but one of these coefficients are positive as hypothesized, but only the two coefficients in the WIN knowledge model are statistically significant at the 10 percent level.

2.3.2 The Effect of Labor Market Characteristics

There is great geographic variability in employer familiarity with CETA-OJT. As hypothesized, familiarity with the program was greatest in rural labor markets. Familiarity was also significantly higher in rural Washington state and Ohio, in labor markets with high proportions of minorities, and a high unemployment rate.

There was less geographic variability in familiarity with the employment tax credit programs. Rural and small city labor markets were significantly more likely to be familiar with WIN and NJTC but less likely to be familiar with TJTC.

Employers in Washington state and Texas were significantly more familiar with TJTC. The employers in Texas were significantly less familiar with WIN. Other geographic variables--the proportion in poverty, the proportion minority, and the unemployment rate--were not significantly related to familiarity with these employment tax credit programs.

2.3.3 The Effect of Outreach and Past Experience

Efforts by local government representatives to promote these programs have a significant impact on employer knowledge of them. Roughly 6 percent of our sample first heard of WIN from a government representative. Would a doubling of these contacts produce an equivalent increase in knowledge or would some of the firms contacted have learned of the program another way? The coefficient on the outreach variable indicates that most of these contacts result in almost equivalent increases in knowledge. They imply that a .06 increase in proportion of those contacted about WIN increases the proportion familiar by .05.

Past receipt of one employment subsidy increases a firm's propensity to know about other employment subsidy programs. Having received an NJTC in 1977 or 1978 increases the proportion of firms that know about TJTC by .20, the proportion familiar with WIN by .14, and the proportion familiar with CETA-OJT by .10.

2.4 Summary and Policy Implications

This examination of the determinants of knowledge about subsidy programs has found considerable support for the theoretical perspective that was advanced at the beginning of the chapter. The firm that is most likely to be familiar with employment subsidy programs is a large, growing, manufacturing firm that has a predominantly white-collar work force and that is a member of a local business organization. Outreach by local administrators of the program had an important impact on employer familiarity. Knowledge of CETA-OJT is greatest in rural sites with a high 1978 unemployment rate and those with a high minority population.

In our view, the primary explanation for geographic variation in familiarity with these programs is variation in local promotional efforts (only some of which were measured by our outreach variable). From this we derive the hopeful conclusion that while ignorance was, in 1980, the major barrier to use of these programs, effective promotional efforts have and can overcome this barrier.

Our finding that past users of one of these subsidies are more likely to be familiar with other programs can be given two interpretations. One interpretation is that the association reflects the effects of an unmeasured propensity to participate in employment subsidy programs. The other interpretation gives the association a causal intepretation: participation in one program lowers the costs of learning about other programs. If the second interpretation is correct, knowledge and participation will grow with time, and the ignorance barrier will eventually be overcome.

CHAPTER 3

PARTICIPATION IN EMPLOYMENT SUBSIDY PROGRAMS

John H. Bishop and Mark Montgomery

3.1 Introduction

This chapter examines what determines whether a firm that is "familiar" with a particular employment subsidy program chooses to "participate" in the program by hiring at least one eligible worker. It begins with a theoretical discussion of how we would expect various characteristics of the firm to effect its decision to participate in targeted recruitment subsidies such as TJTC, WIN, and CETA-OJT, and in general employment subsidies such as NJTC. (These expectations are largely based on a general theoretical model of firm participation in marginal employment subsidies that is presented in Appendix A.) Next we present the empirical model used to study the determinants of participation in a program. The last section of this chapter presents the results of the empirical analysis of participation and draws conclusions.

3.2 Determinants of Firm Participation in Employment Subsidies

Employment subsidy programs attempt to induce firms to increase hiring of particular types of workers by reducing the wage costs of additional workers of that type. A subsidy of, say, 50 percent of the wage of newly hired eligible workers is not equivalent, however, to a 50 percent reduction in the market wage rate of these workers. First, when the program offers a recruitment subsidy such as WIN, TJTC, and CETA or a marginal subsidy such as NJTC, a payment is made only for workers who are newly hired (recruitment) or hired after some threshold employment level has been crossed (marginal). No payment is made for workers already employed by the firm. Second, a firm receives the subsidy only if it applies for the subsidy and verifies the eligibility of new workers for subsidization. Even a firm that is aware of the existence of such a program may not have all the necessary information about which job applicants are eligible and which are not. The cost of obtaining this information, of getting the necessary government certifications, and then of applying for the subsidy may deter some firms from participating in the program.

Some of the costs associated with hiring subsidized workers are fixed, while others depend on the number of workers hired through the program. The fixed costs involve such factors as learning enough about the program to make use of it, making arrangements for the referral of eligible workers, establishing a system to identify these workers in the applicant pool, negotiating an OJT contract with CETA, and the risk that participation will entail closer government scrutiny of hiring practices and tax records. The variable (or "incremental") costs of hiring subsidized workers include the cost of searching for eligible workers, of determining the eligibility of applicants, of obtaining certification of eligible workers, and the risk of hiring workers

who may have lower productivity than other applicants. These variable costs are primarily associated with the hiring of workers through a targeted subsidy program such as TJTC, WIN, or CETA. A general scheme that subsidizes all types of workers, such as the New Jobs Tax Credit, should involve few incremental costs since it is unnecessary to certify worker eligibility.

3.2.1 Impact of Firm Characteristics

The firm's decision to participate in one of these programs will depend on a comparison of the perceived benefits and costs of participation. These benefits and costs depend upon the firm's characteristics, the nature of the local labor market, and the policies of the local agencies that are administering the program.

The benefits of participation--the tax credit or subsidy received--are proportional to the number of eligible workers the firm hires and certifies and the amount of subsidy per eligible worker. The costs of participation are in large measure fixed. Those that do vary with the number of subsidized workers do not rise with the amount of subsidy. Consequently, the net benefits of participation are greatest, and therefore the probability of participating is greatest, at firms (a) that have many openings that eligible workers might fill, (b) that receive large subsidies for each eligible worker, and (c) that have low costs of identifying, certifying, and employing eligible workers. The simple model sketched in the previous paragraphs (formally presented in Appendix A) implies that the following characteristics of the firm will, ceteris paribus, be associated with higher probabilities of participation in a targeted or marginal subsidy program.

1. Total employment.

2. Growth rate of employment.

3. The proportion of the work force in low skill jobs.

4. The turnover of existing unskilled workers. (For the targeted recruitment subsidy programs--TJTC, WIN, CETA-OJT--the more rapid the turnover the more openings are available for new subsidized workers.)

5. The elasticity of demand for eligible labor: the elasticity of demand for the subsidized factor should have a positive effect on participation especially in a nontargeted marginal subsidy such as NJTC. This variable will be related to a number of more easily observable characteristics of the firm such as those listed next. (See appendix for more explanation.)

    i. Elasticity of demand for the firm's product.

ii. Elasticity of substitution between low skilled labor and other factors: programs targeted to low skill workers will encourage such substitution.

iii. High shares of total costs going to unskilled labor.

Obviously, a firm that has no openings in the job classifications for which subsidy-eligible workers might qualify will not participate. The more openings a firm has of the type that eligible workers might fill, the more likely it is to choose to participate. Even when fixed costs of participation are high, the benefits of participating will outweigh the costs if the number of eligible new hires is sufficiently large.

6. The probability that the firm has a tax liability. This proposition does not apply to OJT.

7. Flexibility in terminating unwanted employees. The purpose of these programs is to induce firms to hire workers they would not normally be willing to hire. Most employers feel that hiring a subsidy-eligible worker means they are taking a greater risk that things will not work out. If the firm can easily correct its mistake by firing the worker, the cost of a mistake is minimized. In another chapter of this report we report our finding that rates of discharge for cause are considerably smaller in unionized firms. Thus, we anticipate that nonunion firms that have a low firing threshold will be more likely to participate.

8. Difficulty in finding reliable unskilled workers. When, for one reason or another (i.e., low wage rates, unattractive working conditions, or a tight labor market), a firm is unable to hire and retain the experienced workers it would prefer, it becomes more willing to hire someone without experience and to provide the training itself or to adjust its hiring standards downward. Thus, when unemployment rates are low and/or the firm reports having difficulty finding reliable unskilled workers, it is more likely to participate in targeted programs such as TJTC, WIN, and CETA-OJT. Tight labor markets have a different impact on participation in a nontargeted marginal subsidy such as NJTC. Holding the growth in demand for the firms' product constant, a tight labor market makes it harder to recruit the employees necessary to expand in response to the subsidy thus decreasing the firm's probability of participating in NJTC.

9. The proportion of the local work force that is eligible for a subsidy. If many local workers are eligible the number of eligible job seekers coming to the door will increase and the costs of searching for eligibles will fall.

10. The proportion of workers that are full time. The cost of identifying and certifying an eligible worker is unrelated to the hours worked or wage of that worker. CETA-OJT contracts typically pay a flat 50 percent of total wages in the first six months, so the

benefit rises in proportion to hours worked per week and the hourly wage rate. TJTC and WIN tax credits are 50 percent of wages paid in the first year up to $6,000. Firms that employ part-time workers at or near the minimum wage would not be receiving the full subsidy, so we anticipate that their participation rates in CETA-OJT, TJTC, and WIN would be lower than would otherwise be expected. For marginal employment tax credits such as NJTC with a cap of $4,200 on subsidizable earnings, we hypothesize that the effect would operate in the opposite direction: firms that use many part-time workers would be more likely to participate than firms that traditionally do not use part-time employees.

11. The wage paid to eligible workers. In the three targeted programs examined, the subsidy rises with the wage rate. Thus, holding the firms' hiring standards constant, the incentive to participate rises with the wage rate. Restaurants and bars that pay considerably below the minimum wage because tips provide most of their staff's compensation would have only minimal incentives to participate. For non-targeted marginal subsidies such as the NJTC with a $4,200 cap on subsidizable earnings, low wage rates increase the incentive to participate.

3.2.2 The Impact of Local Marketing Efforts

We also expect the policies of the local agencies administering the program to be very important determinants of the participation rate. This is especially true in the CETA-OJT program where (a) local prime sponsors control the allocation of CETA training funds between classroom training and OJT and, therefore, the scale of program in the community, and (b) staff has discretion over which firms are approached about participating in the program and which firms are presumed to offer sufficient training to warrant receiving a subsidy. Because of the high costs of commuting to a central location for classroom training and the inability of rural labor markets to absorb large numbers of classroom training graduates, the agencies administering CETA funds in rural areas have chosen to emphasize OJT contracts rather than classroom training. Some CETA prime sponsors seem to have a policy of limiting the number of OJT contracts they will sign with any one employer. Where this is the practice, the largest firms have a reduced incentive to participate. Thus, the positive relationship between size and participation that we have predicted for the other tax credit programs may not hold for CETA.

Administration of the TJTC is primarily in the hands of the local employment service offices. In some communities, employment service staff members have marketed TJTC by telephoning local employers and offering to come to their plants to help identify and then certify the TJTC eligibles that were working there. In other communities, employers who seek referrals of eligible workers or more information about the program get no help at all (in Wisconsin, for instance, when the federal contribution to administrative costs ran out in October 1979, certifications dropped to almost zero in the final three months of the year). Firms cannot participate in a program if they do not

know who to contact locally about application and certification. Consequently, we expect that participation as well as familiarity will be greater in communities in which there has been extensive promotion by the local employment service. Similarly, the cooperative relationship between the local employment service office and the firms will have a bearing upon the likelihood of participation. Firms that have regular and frequent contact with the employment service people are more likely to get referrals of eligible applicants.

3.3 Predicting Participation of Firms that Are Familiar with the Program

We saw in chapter 1 that ignorance of the existence of employment subsidies is the primary reason that firms do not participate in these programs. Only about half of all firms were aware of NJTC and CETA programs, while only 29 percent were familiar with WIN and 17 percent with TJTC. Any empirical analysis of the determinants of firm participation needs to separate the issue of what determines which firms are familiar with the program from the question of what determines participation among the knowledgeable firms. To see this, suppose we let $H_i = 1$ represent the event that the i^{th} firm hires workers through the program and $H_i = 0$ be the event that it does not. Let $K_i = 1$ be the condition that the firm is aware of the existence of the program and $K_i = 0$ that it is not. Firms that are unaware of the program are certainly unable to participate. That is, we must assume that

$$(1) \quad \text{Prob}\,(H_i = 1 | K_i = 0) = 0$$

The probability that a randomly selected firm will participate in the program is equal to the product of the probability of participation given knowledge and the probability of knowledge of the program.

$$(2) \quad P_i^H = \text{Prob}\,(H_i = 1 | K_i = 1) * \text{Prob}\,(K_i = 1)$$

If one's purpose is to draw general conclusions about the potential of wage subsidies from data on early experience with the program, it is necessary to treat these probabilities separately. With the passage of time, more and more firms will become familiar with the program, and the Prob $(K_i = 1)$ will start approaching one.

To obtain estimates of the probability of participation conditional upon program familiarity we can simply estimate multivariate probit models of participation (see Appendix B) on a sample of firms from which those unfamiliar with the program are excluded. However, this censorship of the sample may produce a biased estimate of B, the vector of probit coefficients. (A more detailed explanation of the sample-selection problem is presented in Appendix B.) Relatively simple methods for testing and correcting for sample selection bias in the linear regression framework are well documented in the econometric literature (see, e.g., Heckman 1976). This is not the case, however, in the probit framework. Within the scope of this study we are at best able to use a combination of devices to give us an indication of the existence and

severity of any selection bias that might occur. A full description of our indicators of selection bias is given in Appendix B.

3.4 Empirical Results

This part presents the results of our multivariate probit models of participation in TJTC, WIN, CETA-OJT, and NJTC by firms familiar with the program of interest.

The base sample used in this analysis is drawn from the subsample of the EUPP data, which contained 5,279 for-profit firms having some positive amount of employment in December of 1979. The estimation sample contained 4,528 firms that had at least one employee in December 1979 and that answered all of the pertinent questions.[1] Supplementary sources provided some of the data about characteristics of the site. All variables used in this analysis pertain to the time of the interview unless otherwise specified. (Definitions and descriptive statistics for these variables are presented in table 2.1 of the previous chapter.)

Tables 3.1, 3.2, 3.3, and 3.4 present estimates of the probit coefficients for 1979 participation in TJTC, WIN, CETA-OJT, and NJTC respectively, for those firms that were familiar with the program of interest. For the targeted programs, firms were asked if they had (a) "hired any workers through" each program and (b) "received a tax credit" for any worker hired through that program. Because rapid turnover of workers, especially low skilled workers, could cause some firms to lose subsidized workers before a subsidy could be applied for, the first question (did you hire?) was used as the dependent variable for the TJTC and WIN equations. For the CETA programs, firms were asked if they hired workers though CETA programs other than CETA-OJT.[2] Therefore, the "received CETA" question provided the dependent variable for the CETA equations. For NJTC, firms were asked (a) whether they had received a NJTC credit and (b) whether NJTC influenced the establishment to increase total employment. For NJTC, the received question was used as the dependent variable. By these measures the participation rates in the estimation sample were .15 for TJTC, .066 for WIN, and .077 for CETA. Since our respondent was the official responsible for hiring, that person could be expected to know whether the firm had hired through or received a subsidy from one of the targeted programs. Where this respondent was the owner of the firm, that individual would also know whether the firm received a NJTC. Personnel officers of larger firms and managers of branch plants are not, however, generally aware of whether their firms obtained a NJTC. Consequently, the reader should be cautious in interpreting the NJTC participation results.

The first column of each table contains the probit estimates of the effects of firm characteristics upon 1979 participation in the program from a model that excludes measures of past participation in the same or similar programs. The second column of the tables contains the estimates for a model that includes past usage of employment subsidy programs. The final column presents results from a model in which some potentially endogenous variables are instrumented. The t statistics are in parenthesis to the right of the

TABLE 3.1

PARTICIPATION IN THE TARGETED JOBS TAX CREDITS BY
EMPLOYERS THAT WERE FAMILIAR WITH IT
(Probit Models)

	Model 1	Model 2	Instrumental Variable Model
Firm Characteristics			
Employee Growth Rate 1978-79	.22 (1.03)	.16 (.73)	3.06 (1.64)
Proportion Low Skill	.34 (1.46)	.34 (1.42)	1.68 (2.31)
Proportion Part Time	.08 (.28)	.04 (.12)	-2.37 (1.51)
Log Establishment Size	.15 (3.35)	.17 (3.52)	.18 (2.07)
Proportion Union	- .17 (1.02)	- .23 (1.34)	- .33 (1.79)
Firing Flexibility	.40 (3.12)	.39 (2.94)	.36 (2.30)
40 Hours Full Time	.12 (.90)	.15 (1.12)	- .07 (.36)
Reliable Unskilled Workers Not Available	.04 (.32)	- .04 (.35)	.00 (.02)
Market Characteristics			
Unemployment Rate	.06 (1.00)	.08 (1.27)	.07 (1.04)
Proportion Minority	- .54 (.63)	- .94 (1.06)	- .67 (.74)
Proportion in Poverty	-1.37 (.82)	-1.08 (.63)	-2.18 (1.16)
Log Labor Market Size	- .03 (.32)	.03 (.34)	- .03 (.29)
Northwest	- .84 (2.46)	- .73 (2.12)	- .74 (2.01)
Midwest	.06 (.26)	.02 (.09)	- .07 (.29)
Southwest	.00 (.00)	.16 (.46)	.11 (.31)
West	- .21 (.51)	- .24 (.56)	- .25 (.56)
Southeast	- .03 (.10)	.06 (.18)	- .13 (.39)
Outreach Variables			
Learned of WIN from Government Rep.	.45 (3.32)	.34 (2.38)	.35 (2.46)
Member Business Organization	.02 (.13)	.01 (.08)	.00 (.03)
Received WIN 1977 or 1978		1.06 (4.40)	1.01 (4.11)
Received NJTC		.29 (2.34)	.29 (2.24)
Responded to NCTC but Didn't Receive		1.06 (3.41)	.92 (2.92)
Industry Dummies			
Construction	- .34 (1.04)	- .34 (1.00)	.09 (.22)
Wholesale Retail	.25 (1.43)	.22 (1.24)	.26 (1.31)
Finance and Services	.32 (1.87)	.35 (1.97)	.43 (2.27)
Transportation and Communication	- .38 (.96)	- .24 (.60)	- .38 (.94)
2 x Ln Likelihood	91.0	-317.0	-314.9

NOTE: Fifteen percent of the 901 employers familiar with TJTC participated. At the mean participation rate, the multiplier for calculating changes in probability is .23 and for calculating % changes in participation, it is 1.53. Asymptotic t statistics are in parenthesis.

TABLE 3.2

PARTICIPATION IN WIN TAX CREDIT OR OJT PROGRAM IN 1979 BY EMPLOYERS THAT WERE FAMILIAR WITH IT
(Probit Models)

	Model 1	Model 2	Instrumental Variable Model
Firm Characteristics			
Employee Growth Rate 1978-79	.01 (.03)	.03 (.13)	- .33 (.19)
Proportion Low Skill	.97 (3.70)	1.02 (3.60)	1.03 (1.40)
Proportion Part Time	- .30 (.93)	- .25 (.73)	- .32 (.21)
Log Establishment Size	.11 (2.40)	.11 (2.33)	.09 (1.10)
Proportion Union	.03 (.15)	.03 (.15)	.02 (.09)
Firing Flexibility	.50 (3.82)	.51 (3.65)	.50 (3.10)
40 Hours Full Time	.25 (1.71)	.31 (2.00)	.31 (1.46)
Reliable Unskilled Workers Not Available	.07 (.60)	- .04 (.33)	- .02 (.19)
Market Characteristics			
Unemployment Rate	- .16 (2.31)	- .12 (1.60)	- .13 (1.72)
Proportion Minority	.11 (.10)	- .01 (.01)	- .15 (.14)
Proportion in Poverty	-1.52 (.86)	- .59 (.31)	- .62 (.32)
Log Labor Market Size	- .20 (2.14)	- .17 (1.74)	- .15 (1.49)
Northwest	- .01 (.04)	.02 (.07)	.00 (.01)
Midwest	.21 (.89)	.16 (.65)	.13 (.52)
Southwest	-1.05 (2.85)	- .90 (2.35)	- .93 (2.39)
West	- .95 (2.10)	- .67 (1.45)	- .64 (1.40)
Southeast	- .14 (.38)	- .24 (.63)	- .22 (- .56)
Outreach Variables			
Learned of WIN from Government Rep.	.41 (3.30)	.34 (2.60)	.38 (2.92)
Member Business Organization	- .11 (.93)	- .15 (1.13)	- .13 (1.01)
Received WIN 1977 or 1978		1.30 (7.30)	1.24 (7.04)
Received NJTC		.22 (1.58)	.21 (1.46)
Responded to NCTC but Didn't Receive		- .15 (.27)	- .10 (.19)
Industry Dummies			
Construction	- .23 (.61)	- .26 (.66)	- .41 (.94)
Wholesale Retail	.05 (.26)	.05 (.23)	- .02 (.07)
Finance and Services	.20 (1.10)	.19 (.95)	.14 (.69)
Transportation and Communication	- .52 (1.11)	- .61 (1.10)	- .64 (1.17)
2 x Ln Likelihood	114.4	255.0	260.3

NOTE: Out of 1,405 employers familiar with WIN, 6.6 percent participated. At the mean participation rate, the multiplier for calculating changes in probability is .135 and for calculating % changes in participation, it is 2.05. Asymptotic t statistics are in parenthesis.

TABLE 3.3

PARTICIPATION IN THE CETA-OJT PROGRAM IN 1979 BY EMPLOYERS THAT WERE FAMILIAR WITH IT
(Probit Models)

	Model 1	Model 2	Instrumental Variable Model
Firm Characteristics			
Employee Growth Rate 1978-79	.23 (1.23)	.19 (.93)	.78 (.46)
Proportion Low Skill	.23 (1.09)	.27 (1.22)	.44 (.68)
Proportion Part Time	- .33 (-1.16)	- .36 (1.17)	-1.29 (.92)
Log Establishment Size	.10 (2.38)	.09 (1.93)	- .07 (.83)
Proportion Union	- .20 (1.33)	- .18 (1.09)	- .18 (1.07)
Firing Flexibility	.37 (3.18)	.36 (2.88)	.40 (2.72)
40 Hours Full Time	.17 (1.36)	.17 (1.25)	1.05 (.26)
Reliable Unskilled Workers Not Available	.24 (2.20)	.20 (1.74)	.20 (1.67)
Market Characteristics			
Unemployment Rate	- .05 (- .90)	- .06 (.98)	.07 (1.08)
Proportion Minority	.32 (.38)	.35 (.40)	.41 (.41)
Proportion in Poverty	-2.92 (1.83)	-2.90 (1.70)	-3.26 (1.78)
Log Labor Market Size	- .12 (-1.54)	- .14 (1.61)	- .16 (1.77)
Northwest	- .26 (- .95)	- .31 (1.10)	- .29 (.97)
Midwest	.25 (1.09)	.24 (1.02)	.21 (.87)
Southwest	.10 (.34)	- .03 (.08)	- .05 (.14)
West	- .16 (- .46)	- .20 (.54)	- .23 (.64)
Southeast	.04 (.12)	.06 (.19)	.03 (.09)
Outreach Variables			
Learned of CETA-OJT from Government Rep.	.63 (5.96)	.53 (4.74)	.54 (4.80)
Member Business Organization	- .07 (- .70)	- .10 (.85)	- .10 (.83)
Received CETA-OJT 1977 or 1978		1.17 (8.31)	1.13 (7.68)
Received NJTC		.18 (1.40)	.18 (1.36)
Responded to NCTC but Didn't Receive		.30 (.77)	.25 (.64)
Industry Dummies			
Construction	- .02 (- .10)	.05 (.19)	.12 (.37)
Wholesale Retail	- .16 (-1.00)	- .15 (.90)	- .09 (.50)
Finance and Services	- .22 (-1.37)	- .18 (1.06)	- .13 (.70)
Transportation and Communication	- .34 (-1.00)	- .17 (.48)	- .19 (.53)
2 x Ln Likelihood	115.3	311.0	312.2

NOTE: Out of 2,886 employers familiar with CETA-OJT, 7.5 percent participated. At the mean participation rate, the multiplier for calculating changes in probability is .145 and for calculating % changes in participation, it is 1.89. Asymptotic t statistics are in parentheses.

TABLE 3.4

PARTICIPATION IN NJTC FIRMS FAMILIAR WITH THE PROGRAM
(Probit Models)

	1 Received	2 Received	3 Increased Employment In Response to
Firm Characteristics			
Change in Industry Employment	.03 (.37)	.03 (.39)	- .05 (.40)
Dummy: 2 Digit Sic Code	- .05 (.39)	- .05 (.38)	- .05 (.16)
Dummy: Industrial Category	- .02 (.03)	.10 (.13)	- .16 (.15)
Log Establishment Size	.02 (.99)	.08 (3.04)	- .01 (.19)
Size Greater than 250		- .82 (5.12)	- .30 (1.30)
Proportioned Unionized	- .19 (2.12)	- .17 (1.86)	- .41 (2.86)
Proportion Blue Collar	- .03 (.55)	- .07 (.72)	- .08 (.64)
Proportion Part Time	.21 (1.53)	.27 (1.98)	.32 (1.66)
40 Hours Full-time	.04 (.64)	.03 (.47)	.09 (1.00)
Firing Flexibility	.06 (.90)	.07 (.99)	.29 (2.99)
Market Characteristics			
Unemployment Rate	.05 (2.27)	.05 (2.31)	.03 (.72)
Log Labor Market Size	.01 (.19)	.005 (.15)	- .001 (.003)
Northwest	.32 (2.46)	.33 (2.49)	.30 (1.62)
Midwest	.08 (.79)	.10 (1.08)	.14 (1.05)
Southwest	.08 (.60)	.10 (.77)	- .15 (.70)
West	.15 (.96)	.17 (1.07)	- .10 (.40)
Southeast	.08 (.60)	- .07 (.86	- .15 (.12)
Industry Dummies			
Construction	.40 (3.21)	.34 (2.72)	.04 (.21)
Wholesale and Retail Trade	.02 (.21)	- .06 (.59)	- .09 (.65)
Finance and Services	.09 (.91)	.03 (.33)	- .26 (1.84)
Transportation and Communication	- .04 (.25)	- .12 (.67)	- .30 (1.05)
Membership Business Organization	- .03 (.55)	- .03 (.56)	.08 (.94)
Learned from WIN from Government Rep.	.05 (.48)	.06 (.67)	.15 (1.17)
Constant	-1.14 (2.73)	- 1.19 (2.82)	- 1.64 (2.68)
-2 x ln Likelihood	39.86	68.12	42.17
Proportion Responsing	.29	.29	.07
Probability Multiplier	.31	.31	.13

NOTE: The sample size is 2,191. Asymptotic t statistics are in parentheses.

coefficient. The impact of a variable on the probability of participation conditional on knowledge can be obtained by multiplying the coefficient by the multipliers (.23 for TJTC) that are given in the note at the bottom of the table.[3] Percentage changes in participation can be obtained by multiplying the coefficient by the percentage change multiplier (1.53 for TJTC) given in the note. The use of the coefficients and multipliers can be illustrated by calculating the predicted impact of learning about WIN from a government representative on TJTC participation. Multiplying .45 by .23 provides us with an estimate of approximately .10 for the change in probability of participation. The percentage increase in the participation is 69 percent.

$$\left(\frac{.23}{.15} \cdot .45 = 1.53 \cdot .45 = .69\right)$$

3.4.1 Firm Characteristics

Let us begin by examining the effects of firm characteristics. As expected, size has a significant positive effect upon participation in TJTC and WIN programs. In model 1, a firm that is ten times the size of another is approximately 53 percent [(ln 10) (1.53)(.15) = (2.3) (1.53) (.15) = .53] more likely to participate in TJTC, 54 percent [(ln 10) (2.05) (.11) = .54] more likely to participate in the WIN program, and 43 percent more likely to participate in CETA-OJT. For NJTC, size has a positive and significant effect only when a dummy is included for very large establishments. A dummy for size greater than 250 has a strong negative effect in the NJTC model. In large establishments the owners or tax accountants who might have been aware of their firm's receipt of a NJTC were not respondents to our survey. The personnel officers who were our respondents seemed in many cases, to have reported (incorrectly) that the firm did not receive a tax credit.

One firm characteristic that has a consistently positive effect for the targeted programs is the dummy for flexibility in firing workers. This variable takes the value 1 if during the fourth quarter of 1979 the firm laid off, fired, or induced to quit a worker who would have been retained had job performance been better. This result supports our hypothesis that firms that find it more difficult to terminate an unwanted employee are more willing to hire subsidized workers.[4]

In chapter 8 of the monograph, a strong negative relationship is demonstrated between unionization and the proportion of workers that are fired. We have hypothesized that firms that find it costly to fire workers are more reluctant to participate in targeted subsidy programs, so we would expect unionized firms to have lower participation rates. For TJTC, CETA-OJT, and NJTC, the unionization coefficients were negative, as predicted. The coefficients were statistically significant in the NJTC models and the TJTC instrumental variable model. Our hypothesis that firms that have a difficult time finding reliable unskilled workers would be more willing participants in targeted subsidy programs is born out only for CETA-OJT programs.

In chapter 1 we saw that manufacturing firms had higher than average probabilities of participating in a targeted subsidy program and that retail firms have a lower than average probability of participating. An examination of the industry dummies reveals that these associations were due to characteristics (such as size) that are associated with the industry. When these characteristics were controlled for, only a few industries were shown to have significantly different participation rates: finance and services had a significantly greater probability of participating in TJTC, and construction firms had a significantly greater probability of participating in NJTC.

For the targeted programs, we have reason to suspect that the proportion of the firms' work force in low skill occupations, the proportion in part-time employment, and the growth rate of employment may be affected by participation. To eliminate simultaneous-equation bias, these variables were replaced by instrumental variables. The instrumental variables were obtained by regressing the potentially endogenous variables on a set of instruments that included all of the exogenous variables in the participation equations, the relevant change in employment at the national three-digit SIC level in 1979, the proportion of low skill workers who were in blue-collar occupations, a group of industry dummies developed to capture seasonality in employment, and a dummy for the eating and drinking establishments industry. Although this procedure will generate consistent estimates of the parameters in the probit models, the resulting asymptotic t statistics will only be approximate (see Nelson and Olson 1979, Amemiya 1979).

In the instrumental variables models, the effect of the proportion of workers in part-time employment is consistent with the hypotheses that were specified in section 3.2. Firms with large part-time work forces are less likely to participate in targeted employment subsidies but are more likely to participate in NJTC. This result suggests that the cost of certifying subsidized workers under the targeted programs represents a significant deterrent to participation for firms that hire mainly part-time workers. When these costs are negligible (as they would be for a general program such as NJTC), the fact that the subsidy is tied to FUTA wages makes participation more attractive for firms hiring part-time workers.

The hypothesis that firms with a high proportion of low skill jobs would be more likely to participate is also supported. All of the estimated coefficients are positive. They are significant in the instrumental variable model for TJTC and in the noninstrumental variable models for WIN. There is very little support for the hypothesis that growth of employment tends to increase a firm's probability of participation. In the instrumental variable models the coefficient in the WIN equation is negative. Only in the TJTC model does it gain significance at the 5 percent level on a one tail test.

3.4.2 Outreach by Program Administrators

Our results indicate that personal contacts with employers are a very effective method of promoting these programs. Not only does it inform the employer of the program's existence, but it also greatly increases the

probability that a knowledgeable firm will participate. The coefficients on the dummies for "first having learned of WIN from a government representative" imply that such firms are twice as likely to participate in WIN compared to other knowledgeable firms and that they have a 69 percent higher probability of participating in TJTC. For CETA-OJT, such a contact more than doubles the chances of participation. From the equations for the targeted programs, it is clear that the effect of past experience with employment subsidies has a very significant impact upon participation. Firms that received a New Jobs Tax Credit were 43 percent more likely to participate in WIN and TJTC. Firms that had received a WIN credit in 1977 or 1978 were 150 percent more likely to participate in TJTC and 250 percent more likely to participate in WIN in 1979. For CETA, a NJTC credit increased the probability of participation by 34 percent, and a previous CETA credit increased it by 221 percent.

One surprising result in the equations for the targeted programs is the strong positive effect of the dummy for having increased employment in response to NJTC even though no NJTC credit was received. It was hypothesized that this experience would frustrate the employer and reduce the likelihood of participating in later programs. Alternatively, it might be that the variable is picking up the effect of some unobservable factor which strongly influences willingness to respond to these programs. To test this suggestion, the variable was included in an equation predicting participation in WIN in 1977, before such "frustration" could have occurred. If the dummy tended to proxy for some unobservable, it should have had a strong positive effect in that equation. The coefficient was negative and insignificant, however. Consequently, we tend to feel that our original hypothesis should be completely reversed. An unsuccessful attempt to benefit from a subsidy seems to wet the appetite to try again.

3.4.3 Testing for Sample Selection Bias

It was stated in 3.3 that estimation of probit models conditional upon familiarity with the program in question might result in sample selection bias. Two alternative indicators of this bias were examined to gauge the severity of the problem (full explanations of these procedures are provided in Appendix B).

One "test" for selection bias enters into the participation equation a characteristic of the firm that influences the likelihood of familiarity with the program in question but that is believed not to influence the conditional likelihood of participation. If there is a sample selection problem, this variable will not have the zero coefficient that is hypothesized for it. Consequently, a rejection of the hypothesis that this variable is zero, is evidence of a sample selection problem.

The significance of the business-organization variable in each of the familiarity equations in the previous chapter makes it a useful variable in this regard. Membership in a business organization was demonstrated to increase the likelihood of knowledge of subsidy programs. These organizations would presumably try to inform their members about government programs that

might be profitable for the firms. On the other hand, there is little reason to expect that they would attempt to influence a firm's decision whether or not to take advantage of such opportunities. Therefore, the fact of membership should have little or no effect on the conditional probability of participation. This makes the business organization variable a useful one for making the selection bias "test" outlined previously. Although the sign of this variable is negative for three of the four programs (an indication of selection bias), the variable is in no case significant. The WIN and CETA models give the strongest, though still modest, suggestion of bias.

An alternative method of gauging the severity of any selection problems is to estimate the difference in predicted probabilities between a censored and a noncensored sample. As shown in equation (2) the overall probability of participation is equal to the product of the conditional probability of participation and the probability of familiarity. For those firms that were knowledgeable, we can estimate the predicted overall probability (from the complete sample), the conditional probability (from the censored sample), and the probability of familiarity. If there is little or no selection bias, the predicted overall probability should be close to the product of the probabilities of conditional participation and familiarity. As a method of examining the severity of selection problems, we took the estimated conditional probability (from the instrumental variables models) and subtracted from it the ratio of the overall probability to the probability of familiarity. Since the ratio of the latter probabilities yields a consistent estimate of the true conditional probability, this difference can be viewed as a kind of "error" in calculating the conditional probability (see Appendix B). In this way, mean "percent errors" (the "error" divided by the conditional probability) were calculated for the familiar firms for each of the four programs. For the NJTC equation, the mean percentage difference (percentage of conditional participation) was less than 2 percent. For TJTC and CETA, the estimates were 9 percent and 8 percent respectively. The WIN equation demonstrated the most severe problems with a mean percent error of 33 percent. We conclude from this that though some selection bias problems may trouble these results, only in the case of the WIN program are these problems even moderately severe.

3.4.4 Correlation between Predicted Familiarity and Predicted Participation

The probit estimates of the likelihood of familiarity in chapter 2 can be combined with our estimates of the likelihood of participation-given-familiarity to provide useful information about the additional participation that is likely to be generated by an additional unit of promotional activity for each of these programs. By estimating both the probability that a randomly selected firm will know about a program, given a vector of characteristics X^K, and that it will participate once informed, given characteristics X^H, one can predict whether the returns to informing firms about the subsidy are increasing or decreasing in terms of participation. We can then calculate the correlation between these predicted values P^H and P^K. If the correlation is negative, then the firms that were more likely (than average) to be

familiar with the program were less likely to participate. Thus, a promotional effort that doubled the number of knowledgeable firms would be expected to more than double the number of firms participating. (For a more detailed explanation see Appendix B.) A positive correlation implies that the effort would less than double the number of participants. In this way, we can determine whether returns to informing firms about the program are increasing or decreasing in terms of participation rates among knowledgeable firms.

Correlation between predicted probabilities of familiarity with each program and conditional participation in each program (using the instrumental variable models from the targeted programs) were calculated for all firms in the estimation sample. These estimates are reported as follows.

$$r_{TJTC} = .205$$
$$r_{WIN} = .406$$
$$r_{CETA} = .207$$
$$r_{NJTC} = .099$$

Given the normality assumptions behind the estimation of the probit model, the statistic $1/2\ \ln[(1+r_j)/(1-r_j)]$ will be normally distributed with mean $1/2\ \ln[(1+r_j)/(1-r_j)]$ and variance $1/(n-3)$; where n is the sample size (4,528) and r_j is the true correlation for the j^{th} program. (The sample is not entirely random however, a fact that may bias these estimates. Also, in general, sample correlations are not unbiased estimates of true correlations.) The hypothesis that each of these correlations are zero was tested and rejected at the .01 level in every case.

These results indicate that a firm with higher-than-average probability of familiarity with one of these programs has a higher-than-average probability of participation once informed. The implication of this is that were the government to embark upon a promotional campaign that doubled the number of firms familiar with one of these programs, the number of firms participating would increase but would not double. Diminishing returns to expanding familiarity is greatest for the WIN program and smallest for the NJTC program. The firms that are most likely to participate already know about the programs, so efforts to increase usage by increasing familiarity inevitably face diminishing returns.

3.4.5 Policy Implications

This examination of the pattern of familiarity with and participation in employment/training subsidies yields a number of insights into the barriers that have kept participation in the targeted employment subsidy programs at a relatively low level. The single most important barrier is lack of knowledge of the existence and the rules of the programs.

Efforts by program administrators to overcome this ignorance by personal contact with employers are shown to have a significant impact on whether a firm is familiar with and participates in targeted employment subsidy programs. Some states have been very successful in marketing TJTC. At the time

of our interview, Georgia, South Carolina, and Alaska had participation rates that were ten times those of California. The willingness of firms to participate in these programs does not vary appreciably from state to state; what does vary are the policies and commitment of the local administrators of the program. It is the lack of the local employment services commitment to promoting the TJTC program in most parts of the country that is responsible for the low participation. One Oregon employer found his local employment service office ignorant of TJTC and uncooperative. He claimed they were not even set up to certify the eligible workers that he found and hired; he had to go down to the office to teach the staff there how to certify someone.

The other major finding is that the nonpecuniary cost of participating is significant and largely fixed in nature. In our data this results in large firms with many unskilled jobs being more likely to participate. Once a firm has learned how the program works and has developed channels for recruiting eligible workers, the costs of hiring additional eligible workers fall. The result is that while most firms do not participate some of those that do participate hire large numbers of eligible workers. There is, for instance, a janitorial service company in an eastern city that is alone responsible for 1 percent of the entire nation's WIN tax credit claims in the 1978-1979 period. Hiring of subsidy eligible workers is highly concentrated in a few firms. Even though less than 1 percent of all workers are subsidized, the typical subsidized worker is working at an establishment at which 14.6 percent of the firm's employees are subsidized.

It is important to keep the marginal costs of hiring and certifying additional workers low and, if possible, to make them lower. One approach to lowering the costs would be for local employment service offices to offer to screen all job seekers who come through its doors for eligibility if some large local employers will give them standing job orders to fill. It may be that the most efficient way to reduce the structural unemployment of welfare recipients and disadvantaged youth is to encourage what seems to be a tendency for certain employers to specialize in hiring and training this disadvantaged population.

The outreach efforts of local program administrators also need to take into account the greater reluctance of firms that are unionized and offer job security to participate in these programs. There is a trade-off between the quality of jobs a firm offers and its likelihood of responding favorably to a recruitment effort for targeted employment subsidy programs.

NOTES

1. It is possible that exclusion of the firms that did not answer all of the necessary questions could cause same selection bias. There is no simple way to eliminate this problem, however.

2. Many of our respondents seem to have misunderstood the question about hiring through CETA-OJT and reported the hiring of any CETA referral instead.

3. The calculated multiplier is itself an approximation of $F^1(XB)$ generated from a table of standard normal probabilities.

4. It is also possible, however, that this variable is picking up the effects of rapid turnover. Rapid turnover should increase the likelihood of participation in a recruitment subsidy. Also note, however, that the variable was positive and significant in the employment-increase model for NJTC (equation 3, table 3.4). The NJTC program was not a recruitment subsidy and in any case, turnover is not likely to effect actual increases in employment in response to a subsidy. We conclude, therefore, that that firms that have difficulty getting rid of newly hired workers that do not work out seem to be less willing to participate in the targeted programs.

APPENDIX 3A

A Theoretical Model of the Decisions of a Firm Whether to Participate in a Marginal Employment-Subsidy Program

Under the usual assumptions about the behavior of a profit-maximizing firm we expect that if the production function allows for substitution between labor and other inputs, a fall in the wage rate, ceteris paribus, will always result in an increase in labor hiring. Employment-subsidy programs attempt to induce firms to hire more workers by reducing wage costs. A marginal subsidy of, say, 10 percent of wage costs is not equivalent to a 10 percent fall in the market wage, however, since only workers hired beyond some base employment level, set by the program's administrators, are subsidized. Furthermore, marginal subsidy programs require that the firm verify the eligibility of all subsidized workers. The process of applying for the subsidy and certifying eligibility imposes costs upon the firm. Thus a firm may elect not to respond to the offer of a subsidy in return for an increase in its labor force.

This appendix develops a simple theoretical model of the decision of a profit-maximizing firm whether or not to hire any workers through a marginal employment-subsidy program. The model is first used to examine a subsidy of the most general type, wherein the government pays a fixed percentage of the wages of any workers hired beyond some employment threshold. It is then shown how the conclusions of the analysis change when the subsidy is offered as a flat payment on all workers, and/or when the program is targeted to specific types of labor. Finally, a discussion of some of the limitations of the theoretical model is presented.

A. Participation in a General Fixed-Percentage Subsidy

Consider the problem of a firm attempting to maximize profits from the sale of one product in a competitive output market. The firm produces its product with a set of factors purchased in competitive input markets. Labor, l, is one of the factors of production and is substitutable for the other factors. Because the input and output markets are competitive the firm treats all prices as exogenous. Its profit function $\pi(P, W)$ is defined over the wage rate, W, and a vector of all other prices, P. The value of the profit function at (P, W) represents the firm's profit if it optimizes over all inputs and output at those prices.

Suppose that the government offers to subsidize the hiring of any employees hired beyond the base employment threshold $\bar{l}$. That is, if the firm agrees to hire more than $\bar{l}$ workers, the government will pay a fixed percentage, S, of the wages of the $(l-\bar{l})$ additional workers. Thus, the total subsidy payment to the firm, T, is defined as

(1) $T = \text{maximum } [0, SW(l-\bar{l})]$

Suppose further that the government sets $\bar{l}$ as a proportion of the firm's previous employment l_0. Let $\bar{T}$ be set such that $\bar{T} = K\bar{l}_0$, where K is a positive constant.[1]

There is no reason to expect that participation in a subsidy program is costless. Not only must the firm bear the administrative costs of applying for the subsidy and verifying the eligibility of the workers, but the firm may fear that participation will entail closer government scrutiny of hiring practices or tax records. (See Farkas et al.) Thus participation may involve both explicit and implicit costs. Suppose we represent these costs by C and assume that they are a linear function of the number of subsidized workers. That is

$$(2) \quad C = a + b(\bar{l}-\bar{T})$$

where $a, b \geq 0$ and $b < SW$.

When the cost of participation is linear in labor hired, the first order condition for a maximum of profit with respect to labor is the same for the participating firm whether the subsidy is marginal or on all units of labor. Thus we can use the profit function evaluated at a wage of $(1-S)W + b$, after subtracting off participation costs and the subsidy of the first workers, to express profits, πs, when the firm participates.

$$(3) \quad \pi S - \pi(P,(1-S)W + b) - SW\bar{T} - a + b\bar{T}^2$$

Given the conditions assumed, the firm will elect to participate in the subsidy program if it is profitable to do so, that is, it will participate if

$$(4) \quad \pi(P,(1-S)W + b) - SW\bar{T} - a + b\bar{T} > \pi(P,W)$$

For finite values of a and b there will always exist some value of S which satisfies inequality (4). In practice it is reasonable to assume that there are values of S less than unity which will do so, i.e., the government can get the firm to participate with some subsidy rate less than 100 percent of the wage of the marginal worker. Assuming that (P,W) is continuous in W, there exists some critical subsidy rate, call it $\hat{S}$, at which the firm is indifferent between participation and nonparticipation. At $\hat{S}$, inequality (4) becomes

$$(5) \quad \pi(P,(1-\hat{S})W + b) - \hat{S}W\bar{T} - a + b\bar{T} - \pi(P,W) = 0$$

At any subsidy rate larger than $\hat{S}$ the firm will participate and at any subsidy rate smaller than $\hat{S}$, it will not.[3] We can rewrite equation (5) as

$$(6) \quad \pi(P,(1-\hat{S})W + b) - \pi(P,W) = \hat{S}W\bar{T} + a - b\bar{T}$$

To derive an implicit $\hat{S}$ in terms of some characteristics of the firm we can expand the left hand side of (6) into a Taylor Series.[4]

$$(7) \quad \sum_{n=1}^{\infty} \frac{\partial^n \pi(P,W)}{\partial W^n} \frac{(b-\hat{S}W)^n}{n!} = \hat{S}W\bar{T} + a - b\bar{T}$$

From the properties of remainder terms in Taylor Series, we can find some $\hat{W}$, where $(1-\hat{S})W < \hat{W} < W$, such that

$$(8)\quad \frac{\partial\pi(P,W)}{\partial W}(b - \hat{S}W) + \frac{\partial\pi(P,\hat{W})}{\partial W^2}\frac{(b-\hat{S}W)^2}{2} = S\bar{W}\bar{l} + a - b\bar{l}$$

This formulation is convenient because it allows us to use the derivative properties of the profit function (see, e.g., Varian, p. 31) to convert the left hand side of (8) into a more easily interpretable form. These properties allow us to restate (8) as

$$(9)\quad l_W(\hat{S}W-b) - \frac{\partial l\hat{W}}{\partial W}\frac{(\hat{S}W - b)^2}{2} = (SW - b)\bar{l} + a$$

where l_W is the level of labor input which the firm would select at W and $l\hat{W}$ is the level it would select at $\hat{W}$. Dividing through by $(\hat{S}W - b)l_W$ yields

$$(10)\quad 1 - \frac{\hat{n}}{2}(\hat{S} - \frac{b}{W}) = \frac{\bar{l}}{l_W} + \frac{a}{(\hat{S}W - b)l_W}$$

where

$$\hat{n} = \frac{\partial l\hat{W}}{\partial W}\frac{W}{l_W}$$

The $\hat{n}$ is an approximation of the elasticity of labor demand at W.[5] It can be shown that under common assumptions about the nature of the production function $\hat{n}$ is bounded below by the elasticity of labor demand at W and above by the elasticity at $(1-\hat{S})W$. If the elasticity is relatively constant over the range W, $(1-\hat{S})W$ then $\hat{n}$ is a relatively good approximation of n, the elasticity at W. We will hereinafter make this assumption and replace $\hat{n}$ by a simple n.[6]

Equation (10) can be made more intuitively useful by making some assumptions about l_W, the level of labor which would be hired if no subsidy were offered. We have previously defined l_0 to be the labor hired in the period prior to the offering of the subsidy. Suppose we let g be the periodic growth rate in the firm's labor demand in the absence of the subsidy. Then

$$l_W = (1 + g)l_0$$

Substituting this expression into equation (10) produces

$$(11)\quad 1 - \frac{n}{2}(S - \frac{b}{W}) = \frac{K}{1+g} + \frac{a}{(\hat{S}W-b)l_W}$$

Equation (11) represents an implicit function of the critical subsidy rate, $\hat{S}$, in terms of the elasticity of labor demand, the (autonomous) growth rate of labor demand, the fixed and incremental costs of obtaining a subsidy, the wage rate, the number of workers the firm would normally employ (if

no subsidy were offered), and the minimum proportionate increase over previous employment which is necessary to qualify for the subsidy. This critical-subsidy equation gives us a means of seeing how these factors affect the firm's participation decision. For any given subsidy rate, S, offered by the government, the firm participates if $S > \hat{S}$ and refuses to participate if $S < \hat{S}$. We can predict the directions of the effects of the above characteristics upon the likelihood of participation by observing their impact upon the quantity $(S - \hat{S})$. This is accomplished by taking an implicit partial derivative of $\hat{S}$ in equation (11) with respect to the variable of interest, and signing the resultant expression. This is done below for each variable individually.

i) Labor hired at W (the "Size" of the firm): Differentiating (11) with respect to yields

$$(12)\quad \frac{\partial \hat{S}}{\partial l_W} = \frac{-a}{(\hat{S}W-b)l_W^2} \Big/ \left[\frac{-n}{2} + \frac{aW}{(\hat{S}W-b)\,l_W^2}\right] < 0$$

Holding other characteristics constant, the larger the firm is in terms of the size of its normal labor force, the smaller is the minimum subsidy rate required to induce participation. Thus the larger the firm is the more likely it is, ceteris paribus, to participate in this type of marginal subsidy program. It can be seen from (12) that it is the existence of fixed costs of obtaining this subsidy that causes this effect ($a=0 \Rightarrow \frac{\partial S}{\partial l_W} = 0$). There is another reason why these programs may favor larger firms, however. We have implicitly treated the cost parameters a and b as if they were constant across firms. It is possible however that larger firms enjoy economics of scale in personnel management. This would imply that a and b are decreasing functions of l_W since large firms can more efficiently process the application and certification materials. It is straightforward to show that if b is a decreasing function of l_W the sign of $\frac{\partial S}{\partial l_W}$ is negative even if $a = 0$. In the presence of fixed participation costs, and/or economies of scale in processing certification materials, participation in marginal subsidy programs is inherently more attractive to larger firms.

ii) Growth rate: Equation (11) can be used to show that firms which are experiencing more rapid autonomous growth are, ceteris paribus, more likely to participate.

$$(13)\quad \frac{\partial \hat{S}}{\partial g} = \frac{-K}{(1+g)^2} \Big/ \left[\frac{-n}{2} + \frac{aW}{(\hat{S}W-b)^2 l_W}\right] < 0$$

This is an intuitively clear notion since firms which are rapidly growing are more likely to qualify for the subsidy even before they consciously respond to the incentive of lower wage costs. That is, for such firms, l_W is more likely to exceed the threshold. For any given K, if g exceeds K-1, then l_W is greater than $\bar{T}$ and all workers hired in response to the program are subsidized. If g is less than K-1, a portion of any increase in labor

hiring generated by the program will be unsubsidized. Thus, for example, if K equals 1.02 (like the threshold for the New Jobs Tax Credit) and the firm has a zero growth rate, the first 2 percent increase in employment in response to the subsidy offer will cost the firm the full wage. Such a condition would reduce the likelihood of a response to the program.

iii) The wage paid for unit of labor: Differentiating (11) with respect to W yields

$$(14)\quad \frac{\partial \hat{S}}{\partial W} = \left[\frac{nb}{2W^2} \quad \frac{a\hat{S}}{(S-b)^2 l_W}\right] \Big/ \left[\frac{-n}{2} + \frac{aW}{(\hat{S}W-b)^2 l_W}\right] \quad < 0$$

Equation (14) implies that as long as there are fixed and/or incremental costs associated with hiring workers through the subsidy program, high-wage firms will be more likely to participate, ceteris paribus. If the government pays a fixed percentage of the wage cost of the additional workers, than as W rises, the fixed cost represents a smaller portion of the total subsidy payment given the number of workers hired. Also, the effective percentage subsidy, when there are incremental costs, is S - b/W. As W rises, the effective subsidy rate increases. It will be shown in the next section that the effect of the wage rate is sensitive to whether the subsidy is a fixed percentage of the wage or a flat payment of all workers.

iv) Elasticity of labor demand: Firms which have more elastic demands for labor input, ceteris paribus, are more likely to respond to an employment subsidy.

$$(15)\quad \frac{\partial \hat{S}}{\partial n} = \frac{\hat{S}}{2} \Big/ \left[\frac{-n}{2} \quad \frac{aW}{(\hat{S}W-b)^2 l_W}\right] \quad > 0$$

In our context the elasticity of labor demand should be viewed not so much as a characteristic of the firm but as the interaction of various characteristics. Given the prices facing the firm in its input and output markets, the elasticity of labor demand is determined by the firm's production function. Although the production relations cannot be observed directly, the elasticity can be expected to correspond to some observable characteristics of the firm. Below are considered three such characteristics of the firm which we expect on a priori grounds to be related to the elasticity of labor demand, and thus the likelihood of participation.

a) Type of industry: One reasonable prediction is that the wage elasticity varies across types of industries, since production technologies certainly differ widely between, say, manufacturing and service industries. It is difficult to form expectations however about which is greater. Empirical studies of elasticities of labor demand rely almost exclusively upon data for either manufacturing industries or all private-nonfarm employment (see Hammermesh 1975 and Hammermesh and Grant 1979). We cannot predict with any degree of confidence, therefore, which types of industries are more

or less likely to participate in employment-subsidy programs. It is expected, however, that variation among industry types will occur.

b) Distribution of the workforce among types of workers: It is reasonable to expect that the distribution of the firm's labor force among types of workers will have a significant effect upon the wage elasticity of labor hiring. Studies of short-run elasticities of labor demand which calculate values for both blue and white collar workers are not in agreement about which is the higher value, although they all seem to produce significantly larger elasticities for one than for the other (see Hammermesh and Grant 1979). Thus, firms that differ in the proportion of white vs. blue collar employment may differ in the likelihood of participation of subsidy programs, but the direction of the difference is unclear.

Studies of elasticities for different age categories of workers by Anderson (1977) and Grant (1979) report values for the fourteen to twenty-four age categories which are more than twice as high (in absolute value) as for older workers. Firms with high proportions of very young workers would be expected to have a higher overall wage elasticity. It is quite possible that some of this difference is attributable to variations in skill levels between younger and older workers. We might, therefore, expect that firms with higher proportions of young and/or unskilled workers would, ceteris paribus, be more likely to participate in marginal subsidy programs.

Summarizing briefly, the theoretical model predicts that the likelihood of participation in a "fixed-percentage", non-targeted marginal employment subsidy will vary directly with the size of the firm (in terms of its labor force), the autonomous growth rate of labor demand, the wage paid per unit of labor, and the elasticity of labor demand. It was argued that the elasticity of labor demand would be related to factors such as unionization, distribution of labor force among occupation and skill classes, and type of industry. The degree of unionization was expected to vary inversely with the likelihood of participation. The proportion of workers in lower skill classes was expected to be positively related to participation, and the variation across industry types was considered uncertain.

B. Extension of the Model to Alternative Program Designs

The theoretical model presented in part A related to a subsidy which paid a fixed percentage of the wages of any subsidized worker brought into the firm. The subsidy was offered on all types of workers. In fact, all labor was implicitly considered homogeneous. Though this serves as a useful benchmark in evaluating employment subsidies, in practice these programs vary a great deal in design. As stated earlier, many subsidies, like the WIN and TJTC programs, are targeted to particular categories of workers. Some programs offer a flat payment on subsidized workers, such as the French Incentive Bonus for Job Creation and the British Small Firms Employment Subsidy. Even the NJTC, TJTC, and WIN programs, which pay fixed percentages of wages below the FUTA wage base, become flat-payment subsidies above the level of wages covered unter FUTA. (For more information on all of

these programs, refer back to chapter 1.) For these reasons, it is important to consider how sensitive the conclusions in Part A are to the distinctions between fixed-percentage and flat-rate subsidies. Under heading i) the distinction between targeted and non-targeted subsidies is drawn.

i) Flat-payment subsidies. Suppose the government offers a payment of a fixed dollar value for any subsidized workers hired. We can restructure equation (10) to represent a critiical subsidy payment rather than a critical subsidy rate. The term "$\hat{S}$" is now used to signify a dollar value and is implicitly defined as follows.

$$(16) \qquad 1 - \frac{n}{2}\frac{(\hat{S}-b)}{W} = \frac{K}{1+g} + \frac{a}{(\hat{S}-b)l_W}$$

Following the method used in section I we can differentiate $\hat{S}$ with respect to the growth rate, the size of the firm, the elasticity of labor demand and the wage paid to workers. Only in case of the wage do we observe a reversal in the direction of the effect when the subsidy payment becomes flat.

$$(17) \qquad \frac{\partial S}{\partial W} = \frac{-n}{2W^2}\left[\frac{-n}{2W} + \frac{a}{(\hat{S}-b)^2 l_W}\right] > 0$$

The effects of the other variables have the same sign as in the case of a fixed percentage subsidy. The reversal of the effect of the wage rate upon the likelihood of participation, when a subsidy is a flat payment as opposed to a fixed proportion of the wage, is intuitively quite reasonable. Given incremental costs b and payment S, the effective subsidy rate, (S-b)/W, falls with W. High wages imply lower percentage reductions in wage costs for any given payment.

ii) Targeted vs. general subsidies. The theoretical model presented in part A relates to a marginal subsidy on all types of workers. The subsubsidy paid by the government reduced the wage, W, paid to the implicitly homogeneous input labor. To apply the model to a targeted wage subsidy we could simply define a set of wages W, W = (W_1, W_2....), and let the subsidy pertain only to W_i, the wage paid to the i^{th} type of labor. It is straight forward to show that the only adjustment to our conclusions is that the variables g, n, W and l_W in equation (11) are now the growth in demand, wage elasticity, wage, and pre-subsidy level of hiring, respectively, of targeted labor. As it is structured, the model can be applied to a marginal subsidy on any factor of production. There are important differences between targeted and non-targeted programs, however, which the model does not capture.

Because targeted subsidies apply only to specific groups of workers, employers must verify the eligibility of each candidate on an individual basis. This raises the incremental cost of hiring subsidized workers (b in our model) very substantially. Also, because targeted workers are generally in population minorities the cost of "searching" for an eligible worker is

much higher under a targeted program. Finally, the very fact that a worker is eligible for a special subsidy may stigmatize his or her in the eyes of a prospective employer. The perceived risk associated with hiring these workers can be considered an addition to the incremental cost of participation.

The arguments presented above suggest that a primary distinction between targeted and nontargeted subsidies can be subsumed in the cost parameter b. Targeted programs have significant incremental costs while general programs do not. Increases in these incremental costs reduce the likelihood of participation. From (11)

$$(18)\quad \frac{\partial \hat{S}}{\partial b} = W > 0$$

The theoretical model assumed implicitly that b was constant across all types of firms (though it was admitted that larger firms might be more efficient in personal matters and thus have lower incremental costs). When a subsidy is targeted, however, these costs might be influenced by various characteristics of the firm and its location.

Larger firms not only have larger personal staffs, they also have larger applicant pools. This implies that larger firms will encounter targeted workers more frequently than smaller firms; a fact which should reduce the cost of "searching" for targeted applicants.[7] Furthermore, large firms will presumably have greater experience in dealing with all types of workers including those in targeted population groups. This greater experience should tend to reduce the perceived risk of hiring eligible workers. The expected positive effect of size upon participation should, for these reasons, be even greater in the case of a targeted wage subsidy.

Certain characteristics of the firm's location should also influence the incremental hiring costs. Because targeted workers generally fall into small subsets of the population, the relative density of these groups in the local populace should affect the cost of funding eligible workers and thus the likelihood of participation. This factor might also affect the experience the firm has in dealing with targeted workers and thus the perceived risk of firing them. Also, the responsibility for assessing the eligibility of candidates is usually that of the local Job Service office. Sometimes their offices make a point of evaluating all of their worker clients for eligibility. A firm operating in a locality which enjoys a program enthusiastic Job Service office will be more likely to encounter applicants who are already certified (and can so inform a prospective employer). Firms in such areas should experience lower costs of participation. Finally, we should note that the size of the unemployed labor pool should, ceteris paribus, help determine the difficulty the firm has in getting additional workers. The local unemployment rate should, therefore, have a positive impact upon participation.

iii) Limitations of the theoretical model. Any attempt to relate the theoretical results described above to employment subsidies as they would

affect the behavior of actual firms needs to take account of two limitations of the model presented above. First, it should be remembered that labor in this model was implicitly treated as though it were infinitely divisible. In fact, even when hiring only a part-time worker, the firm pays the same certification costs as for a full-time worker. For very small firms, an added full-time worker may represent a substantial rate of growth. Thus very small firms may be less likely to participate in these programs because they are likely to buy labor services in relatively small quantities.

A second limitation of this model is its assumption that the firm operates in competitive labor markets. The wage rate facing the firm for any type of labor is fixed. Not all firms face horizontal supply curves of labor. A monopsonist in a labor market would be less likely to participate in an employment subsidy program since additional hirings would require that the firm pay higher wages. It should also be noted, however, that the existence of a flat labor supply is consistant with a situation which requires employment subsidy. If wages were free to adjust completely we would presumably witness full employment. This limitation, therefore, is a relatively minor one.

Finally, it should be noted that this model assumed a threshold employment level which the firm needed to satisfy before subsidization began. Programs such as TJTC, WIN and CETA are actually recruitment subsidies. They pay the firm a subsidy (tax credit) on any newly-hired workers. If a recruitment subsidy somehow prevents a firm from replacing workers already hired with new subsidized workers, then the recruitment subsidy is a special case of marginal subsidy; one for which $K = 1$. In practice, this is very difficult to accomplish. Also, firms experience genuine turnover of employees, especially low-skill employees. Thus, new workers are hired regularly without necessarily implying an increase in labor hired. The difference between a true marginal subsidy and a recruitment subsidy is, therefore, an important distinction which this model fails to account for.

NOTES TO APPENDIX 3A

1. This is a standard technique for updating the subsidy threshold. The New Jobs Tax Credit, for example, subsidized that part of the firm's wage bill which was in excess of 102 percent of the previous year's wage bill.

2. The first order condition for a maximum of profit with respect to labor for the firm hiring more than $\bar{T}$ units of labor is the same whether the subsidy is marginal or on all labor. Therefore, all input levels are the same, ceteris paribus, and we need only subtract out the subsidy not paid to the first $\bar{T}$ units of labor from the value of the profit function evaluated at the subsidized wage. If there are costs associated with participation, we evaluate the profit function at the effective subsidized wage ((1-S)W+b) and subtract off the fixed cost. This formulation can only be used, however, if we assume that the cost-of-participation function is linear in labor. That is, we must assume that $\frac{d^2C}{dl^2} = 0$.

3. This follows from the fact that $\frac{\partial \pi}{\partial W} < 0$ if labor hiring is greater than zero.

4. In his study of worker participation in the Seattle and Denver Income Maintenance Experiments, Ashenfelter (1980) used a similar technique involving the individual's expenditure function. The idea of employing the profit function in a model of firm participation in employment subsidies occured to me after reading Ashenfelter's paper.

5. It should be noted that if we follow the common practice of throwing out of the Taylor series terms of order higher than 2, we get exactly the elasticity of labor demand at W.

6. It can be shown that if we make some reasonable assumptions about the production function $\hat{n}$ will lie between the values of the elasticity at W and at $(1-\hat{S})W$. What we need to assume is that $f'''(l)>0, \forall l>0$. The common assertion that the marginal product of labor approaches zero asympototically as l grows without bound requires this same condition on f'''. Thus our claim is not far removed from standard production theory.

The assumption about the relative constancy of the elasticity of labor demand is also not unusual in production theory. The restricted Cobb-Douglas function is a common example of an insolastic technology. (See, e.g., Varian, p.21).

7. Upon close examination this argument can be seen to rely upon the indivisibility of labor. Suppose 10 percent of unemployed laborers are targeted, and therefore 10 percent of all job applicants are targeted. A firm which gets ten applications per week will receive an average of one application per week from a targeted worker. A firm with one applicant per week will not have 10 percent of its applicant pool fall into the targeted category each week, however. The one worker it sees will either be targeted or not. It may wait weeks before a targeted worker applies.

APPENDIX 3B

Econometric Methodology and Related Issues

This appendix discusses the econometric issues relevant to the estimation of the empirical models. Section 3.B.1 describes the multivariate probit model. Section 3.B.2 discusses sample-selection-bias and section 3.B.3 discusses the correlation between predicted familiarity and predicted conditional participation.

3.B.1 The Probit Model

This section derives the empirical specification of our models of familiarity with and participation in employment subsidy programs. The multivariate probit model provides a useful framework for determining which factors contribute to the likelihood of one outcome in an event with two possible outcomes. Suppose we wish to predict whether a particular firm (the i^{th} firm) will participate in a given employment subsidy program, given a vector of the firm's characteristics X_i, $X_i = (X_{i1}, X_{i2} \ldots X_{iK})$. The firm is assumed to participate in a specific subsidy program if the net benefits of doing so are positive. Suppose further that the net benefits NB_i are a linear combination of the characteristics X_i and an error term. That is

$$(1) \quad NB_i = X_i'B + \varepsilon_i$$

Where B is a vector of constants and ε_i has mean 0 and variance σ^2. If the number of variables contained in X_i is sufficiently large then NB will be approximately normally distributed (by the Central Limit Theorem for Sums of Random Variables.)

The firm participates in the program when net benefits are greater than zero. Thus the probability of participation, P_i^H, is such that

$$(2) \quad P_i^H = \text{Prob}(NB_i > 0) = \text{Prob}(X_i'B + \varepsilon_i > 0)$$

The error term ε_i will be normally distributed (given X_i) and it is straightforward to show that

$$(3) \quad \text{Prob}(X_i'B + \varepsilon_i > 0) = \text{Prob}(\varepsilon_i > -X_i'B) = F(X_i'B^*)$$

where B^* is the vector B normalized by σ^2: and $F(\cdot)$ is the cumulative normal distribution function:

$$(4) \quad F(X_iB^*) = \int_{-\infty}^{X_iB^*} \frac{1}{2\pi} \exp(-V^2/2)\, dv$$

Thus, we can define p_i^H to be such that

$$(5) \quad p_i^H = F(X_i'B^*)$$

An estimate of the vector B^* (referred to hereinafter as simply B) in (5) can be obtained by the maximum-likelihood technique using a sample of individual firms for each of which we have information about the vector X_i and whether or not the firm participated in the program in question.[1]

This is accomplished by choosing a $\hat{B}$ to maximize the log of the likelihood function:

$$(6) \quad L = \sum_{i \in R} \ln F(X_i B) + \sum_{i \in R_0} \ln(1-F(X_i \hat{B}_i))$$

Where R_1 and R_0 are sets containing the observations with, respectively,

$$p_i^H = 1 \text{ and } p_i^H = 0$$

We can use this estimate, call if $\hat{B}$, to predict the impact of a change in one of the X variables upon the probability of participation.

$$\frac{\partial p_i^H}{\partial X_{ij}} = F'(X_i B)^* B_j, \text{ where } X_{ij} \text{ is a component of } X_i.$$

3.B.2 Sample-Selection Problems in Estimating the Likelihood of Participation Conditional Upon Familiarity

Suppose it is our intention to examine how various characteristics of the firm and its location will influence the likelihood that it will participate in a subsidy program about which it has been informed. (Which is what we do in chapter 3.)

It is possible to estimate the probability p^H, conditional on familiarity and characteristics X_i^H, that a firm will choose to hire a subsidized worker, and the probability p_i^K, conditional on X_i^K, that it is aware of the program in question using a multivariate probit approach described above.

$$(7) \quad p_i^H = F(X_i^{H'} B^H) + \varepsilon_i^H$$

$$(8) \quad p_i^K = F(X_i^{K'} B^K) + \varepsilon_i^k$$

Where all notation is as in 3.B.1.

Suppose we define $K_i=1$ to be the event that the i^{th} firm is familiar with the program and $K_i=0$ be the event that it is not; and let $H_i=1$ be the event that it participates and $H_i=0$ be the event that it does not. Our stated intention in chapter 3 is to estimate the probability of participation given familiarity with the program in question. Rather than estimate equation (7), we want to estimate

$$(9) \quad p_i^H = \text{Prob}\ (H_i=1|K_i=1) = F(X_i^H B^H|K_i = 1) + \varepsilon_i$$

We can estimate B^H in (9) by using a probit model on a sample which only includes firms which were familiar with the program in question (those for which K = 1). Since only firms which are familiar with the program are part of the sample which estimates (9), the probability of participation is estimated on a censored sample. This raises the possibility that sample-selection-bias problems will emerge if there are unobservable factors which jointly influence both knowledge of and participation in employment subsidy programs.

To see this, suppose that there exist unobservable factors which influence both p^H and p^K. Because they are unobservable, their effects will be confined to the error term, implying that ε_i^H and ε_i^K have a nonzero covariance. For simplicity let's assume that the covariance is positive; that is, that firm's which have an unmeasured tendency to be more aware of programs like these are also more likely to participate in them given their level of knowledge.

$$(10) \quad \text{Cov}\ (\varepsilon_i^K, \varepsilon_i^H) > 0$$

The estimate of B^H, call it $\hat{B}^H$, is produced using only firms which know about the program. Firms with positive values of ε_i^K are more likely to know and therefore more likely to be included in the sample which estimates $\hat{B}^H$. For any firm randomly drawn from that sample therefore, $E\ (\ ^K|K = 1) > 0$. But ε_i^K and ε_i^H are positively correlated. This implies that

$$(11) \quad E(\varepsilon_i^H|\varepsilon_i^K > 0) > 0$$

The expected value of ε_i^H is nonzero for firms in the hiring sample, and therefore, the estimate $\hat{B}^H$ is biased. This bias will persist even in very large samples.

To test for the existence of such unobservable influences, we need to discover a component of X^K which is a significant determinant of knowledge, p^K, but can be expected to have no influence on participation given knowledge, p^H. This variable can be included in the equation which estimates p^H, and if it appears to have a significant effect we have strong reason to be suspicious of the existence of some latent variables.

To see this, suppose that there exists some unobservable Z_i which positively influences both P^H and P^K, and some Y_i, a component of X^K, which affects knowledge but has no impact on hiring. For illustration, suppose Y_i affects knowledge positively. A firm with a low value of Y_i should be less likely to appear in the hiring sample. The presence of such a firm in that sample in spite of a low Y_i indicates the existence of some latent variable which makes knowledge more likely. But this variable also positively influences the hiring decision. The combination of a low value of Y and appearance in the participation sample is more likely, therefore, to be associated with a high value of P^H. Thus, Y will have a significant negative coefficient in equation (9) despite the fact that it should have little or no influence on P^H. A result that would calm our fears about latent influences and the attendant selection bias, would be a coefficient of Y in equation (9) which had a value very close to zero (relative to the other coefficients) and had a very small t statistic. If either of these conditions failed, the test for selection bias in the participation equation would be regarded as inconclusive.[2]

The procedure described above does not constitute a "test" for selection bias as we normally conceive of statistical tests. Indeed, simple procedures for testing selection bias in the probit framework have yet to be developed. It is useful, therefore, to have an alternative indicator of the presence of selection bias in this type of model. A second approach to this problem is described below.

From elementary probability theory, we can see that, given a vector of firm characteristics X_i, the probability that the i^{th} firm will participate in a given program is such that

$$(12)\quad \text{Prob}\,(H_i=1|X_i) = \text{Prob}\,(H_i=1|K_i=1,X_i)*\text{Prob}(K_i=1|X_i)$$

Suppose we let the vector X_i contain both X_i^K and X_i^H; that is, all variables affecting either knowledge or participation-given-knowledge. We can then estimate a reduced form probability of participation, conditional on X, for all firms. (That is, we can estimate the left hand-side of (12). The predicted, $\hat{p}_i^H$, from this estimation gives a consistent estimate of the probability that a randomly selected firm will participate in the program. If we also estimate the probability of knowledge, $\hat{p}_i^K$, for all firms, we can use equation (12) to give a consistent estimate of the conditional probability of participation, $\hat{p}_i^{CH}$. From (13) we can see that

$$(13)\quad \hat{p}_i^{CH} = \hat{p}_i^H / \hat{p}_i^K$$

An alternative estimator of $\hat{p}_i^{CH}$, and one which will reveal structural relationships between $\hat{p}^{CH}$ and X for participating firms, is that predicted $\hat{p}_i^{CH}$ which results from a probit model of Prob $(H_i=1|K=1,X_i)$. That is, we can produce another estimate of $\hat{p}_i^{CH}$, call it $\hat{p}_i^{CH'}$, from predicting participation for those firms, and only those firms, which were familiar with the program. (This is the model of interest to us in this chapter.) If

selection bias is a serious problem in the estimation of $Prob(H_i=1|K=1,X_i)$ then we should observe substantial differences between the alternative estimates $\hat{p}^{CH}$ and $\hat{p}^{CH'}$ for those firms familiar with tne program ($\hat{p}^{CH'}$ does not exist for the unfamiliar firms).

To make an alternate "test" for selection bias, estimates of $\hat{p}_i^H$, $\hat{p}_i^K$, and $\hat{p}^{CH'}$ were made for eacn program. The ratio of $\hat{p}_i^H$ to $\hat{p}_i^K$ was compared to $\hat{p}^{CH'}$ for each familiar firm. This allowed us to calculate a mean "percent error" in the estimate of the conditional probability.[3]

3.B.3 Predicting the Returns to Promotional Effort: The Correlation Between Predicted Familiarity and Predicted Participation

Because many firms are slow to discover the existence of employment subsidies, the takeup and therefore the impact of a subsidy in the early periods of its implementation depends upon the effectiveness of the efforts to inform employers of its existence. Because these efforts are costly, it is useful to have information about the additional participation which is likely to be generated by additional promotional activity. By estimating both the probability that a randomly selected firm will know about a program, given a vector of characteristics X_i^H, and will participate once informed, one can predict whether the returns to informing firms about the subsidy are increasing or decreasing in terms of participation.

To see this note that if we can predict p_i^H and p_i^K for any firm if given X_i^H and X_i^K, we can estimate the covariance between these predicted values p^H and p^K respectively, over a sample of firms (or, normalizing by their variances, the correlation of p^H and p^K). The covariance of predicted values $\hat{p}^H$ and $\hat{p}^K$ is the expected value of the product of their deviations from their respective means $\overline{p}^H$ and $\overline{p}^K$.

$$(14) \quad Cov\ (\hat{p}^H, \hat{p}^K) = E[(\hat{p}^H - \overline{p}^H)\ (\hat{p}^K - \overline{p}^K)]$$

A positive covariance implies that when p^K is above its mean, p^H is expected to be above its mean and vice versa. That is

$$(15) \quad Cov\ (\hat{p}^K, \hat{p}^H) > 0 \Rightarrow E[(\hat{p}_i^H - \overline{p}^H)\ (\hat{p}_i^K - \overline{p}^K) > 0] > 0$$

Thus, a positive covariance suggests that firms which were more likely than average to know about the program were more likely than average to participate. Firms with high p^K will be concentrated among the knowledgable group. But high p^K implies high p^H. The firms in the unknowledgeable group will have lower p^K and p^H. Thus a promotional effort which doubles the number of participating firms should less-than-double the number of participant firms. This implies that the returns to promotion of the program are falling in terms of participation rates. A negative covariance (correlation) would imply that returns are increasing.

NOTES TO APPENDIX 3B

1. If we let d_i of the i^{th} firm participated and $d_i=0$ if it did not, the relevant likelihood function for the sample of firms is:

$$L = \prod_{i=1}^{n} F(X_i' B)^{d_i}[1-F(X_i' B)]^{1-d_i}$$

Maximizing the log of this function with respect to B gives asymptotic efficient estimates of B.

2. I am indebted to Gary Chamberlain for suggesting this procedure.

3. This procedure resulted from a discussion with Chris Flinn whom I wish to thank.

CHAPTER 4

THE IMPACT OF TARGETED EMPLOYMENT SUBSIDIES ON THE FIRM'S TOTAL EMPLOYMENT

John H. Bishop and Mark Montgomery

In this chapter we examine the effect of targeted employment subsidies--TJTC, WIN, and CETA-OJT--on the level of employment of firms that use the subsidy programs. The primary purpose of targeted employment subsidies is to change who is hired, not to increase the participating firm's total employment. Nevertheless, theory predicts that targeted subsidies tend to increase employment for two reasons. First, they lower the marginal costs of certain types of labor--tax credit eligible workers. The incentive to expand employment is the greatest when the wages of these types of workers are a major share of total costs or the firm is able to easily substitute these workers for capital or purchased inputs. The second effect of targeted employment subsidies on employment comes through their effect on cash flow. These subsidies increase the firm's cash flow, so small firms whose expansion has been constrained by lack of working capital may find that the increase in profits generated by the tax credit releases this constraint and allows the firm to expand.

Two types of evidence of employment impacts are examined. Section 4.2 reports the responses that were received from employers when they were asked whether they expanded employment because of the programs. Econometric estimates of the impact of the subsidy programs on participating firms' employment growth are presented in section 4.3 and 4.4. First, however, the reader must be warned against overinterpreting the results of the analysis to follow.

4.1 A Warning Against the Fallacy of Composition

Before beginning, it is important to warn the reader against the temptation of adding together the measured program impacts from participating firms and treating that number as the measure of the impact of the program on the economy's employment level. The general equilibrium effects of such a program cannot be calculated in so simple a manner. Finding that the subsidy has caused participating firms to expand is neither a necessary nor sufficient condition for concluding that the program is successfully stimulating total employment in the economy. It is not a sufficient condition because firms compete in both product and labor markets so an expansion by one firm may cause a contraction by another. Firms also buy products and services from one another so an expansion by one firm may also generate extra employment at other firms by increasing demand for their products or services. We suspect the first of these effects outweighs the second. It is not a necessary condition because targeted employment subsidies do not have to increase the employment of participating firms to increase total employment in the economy. Their primary purpose is to induce employers (a) to hire workers with less

skill and experience than they would without the incentive and (b) to provide the more intensive training these new hires require. Even if the firm does not increase its employment, total employment in all firms may expand if the disadvantaged worker who is hired because of the subsidy would not have been able to get a job without its help (because of the minimum wage or some other imperfection in the market) and the less disadvantaged worker who is displaced does find another job because he/she is part of a labor market in which wage rates adjust up and down to equilibrate demand and supply (Johnson 1981).

With these caveats understood we will now turn to the evidence. We will first discuss the employer responses to a direct question about whether they increased employment because of the subsidies. We also have data on actual employment growth rates for different time periods: July through December 1979 and December 1979 until the interview date. Since no measures of subsidy usage are available for 1980 the specification of the models varies with the time period of the dependent variable.

4.2 Increases in Employment Reported to be Subsidy Induced

The EOPP/TJTC Survey asked employers reporting that they hired subsidized employees, "Did participation in the program we just talked about influence this establishment to expand total employment by more than might otherwise have been done?" If they responded "yes," they were then asked, "Approximately how many additional employees were hired that wouldn't have been hired otherwise?" In this section we analyze the responses to these two questions.

Before undertaking this analysis, however, we want to caution the reader against taking the answers to these questions too literally. We anticipate that the responses to these questions will be biased toward zero or no effect. There are a number of reasons for this expectation. Many survey respondents were not owners of the firms and thus were not the people who decided whether total employment was to contract or expand. Despite this, the question elicited only one don't know. We suspect that to many respondents, "no" meant "no, not to my knowledge." Secondly, even when the respondent was the owner, that individual may not have realized the influence a subsidy program was having on decisions. Tax credits and payments for CETA-OJT contracts increase a firm's cash flow. It is not hard to see how improved cash flow could cause a firm to expand without the owner realizing how much of that improvement is due to a wage subsidy scheme. When budgeting for a bid on a construction contract, a firm might take anticipated tax credits into account and thus be more likely to win the contract. Nevertheless, respondents would quite rightly respond to our question by saying, "no, we expanded employment because we won the contract."

It is for reasons such as this that economists always prefer to judge the impact of a policy by making inferences from observation of behavior rather than by asking decision makers why they did what they did. The classic example of how misleading answers to why questions can be is the surveys conducted about business investment decisions. Almost all respondents reported that interest rates are never decisive (White 1956). Despite that, econometric

work on investment consistently obtains statistically significant coefficients on the interest rate or cost of capital variables containing it.

With these caveats understood, we can now examine our data on subsidy-induced increases in employment. A total of 313 firms received subsidies from one of the three programs. Eighty-three firms responded that they were influenced to expand total employment. Of these, three did not know how many extra workers were hired, and four said no extra workers were hired. Thus, seventy-nine establishments, 25 percent of those receiving subsidy, reported an increase in employment due to the subsidies. More relevant is the ratio of the total increase in employment to the total numbers subsidized, which is 20.2 percent.

Should 20.2 percent be considered gratifyingly large or disappointingly small? Comparisons with other surveys and other questions may help the reader make this judgement. In the classic Oxford Economist's Research Group Survey about investment decisions, for instance, only 25 percent of the firms said the cost of capital was a factor in investment decisions (White 1966). In a 1949 McGraw-Hill survey of large firms, only 7 percent said that a one-third decrease in the cost of equity capital would induce them to expand capital investments. Note that a finding that the increase in employment induced by a subsidy of 20 percent of the total number of subsidized workers is a much stronger result than a finding that 20 percent of the firms were "influenced" by a subsidy or that 20 percent did increase employment somewhat. Considering all of the above, it is the author's opinion that 20.2 percent is remarkably high.

In table 4.1 we examine how the net employment ratio varies with the characteristics of the firm. The ratio was 37 percent when the firm received only the WIN tax credit, 16 percent for CETA-OJT alone, and 22 percent for recipients of TJTC alone. Firms that combined WIN with CETA had a net employment ratio of 13 percent, and firms that combined TJTC with one other scheme had a ratio of 18 percent. Firms that used all three subsidy schemes simultaneously had a net employment ratio of 26 percent. These firms were also the heaviest users of subsidy schemes, employing an average of 22.6 subsidized workers or 5 percent of their total employment.

Table 4.1 also allows us to examine the relationship between establishment size, industry, and the net employment ratio. Care must be taken in interpreting this raw data, for one large user of subsidy programs can dominate a particular ratio. This is less of a problem for small establishments and for industries dominated by small firms. The data seem to indicate that small establishments and industries, such as wholesaling, professional services, and retailing other than restaurants that are predominantly small firms, have the highest net employment ratio.

4.3 July to December 1979 Employment Change

This section contains the results of regression experiments relating employment growth between July and December 1979 to the level of combined usage

TABLE 4.1

RATIO OF REPORTED INDUCED INCREASE IN EMPLOYMENT TO SUBSIDIZED EMPLOYMENT

Subsidy Program		Employees in Establishment		Industry	
TJTC only	22%	1-4	44%	Construction	12%
WIN only	37	5-19	31	Low Wage Manufacturing	11
CETA-OJT	16	20-49	7	Mining, Transportation, Utilities, and High Wage Manufacturing	10
WIN & CETA-OJT	13	50-199	19	Wholesale	56
TJTC & one other	18	200-500	31	Retail	28
All three	26	500+	4	Eating and Drinking	15
				Finance	11
				Personal, Business, and Repair Services	18
				Professional Services	25
Full Sample	20.2%	Full Sample	20.2%	Full Sample	20.2%

of CETA, WIN, and TJTC in 1979, and to the change in level of usage from 1978 to 1979. We would have preferred, of course, to study employment change over the twelve-month period from December 1978 to December 1979. The necessary retrospective question was not asked, however. The change was normalized by the average of the employment levels at the endpoints (and converted to a percent). Division by the average level of employment has the advantage of constraining the proportionate change to between plus and minus two, a useful restriction when dealing with very small firms.

The impacts of two aspects of firm usage of the programs upon employment growth were considered. The change in employment was related respectively to the percentage of subsidized employees in the firm's labor force (again, a percentage of the average of the endpoints) and to the change in that percentage from 1978 to 1979.

Theory tells us that the level of a subsidy influences the equilibrium level of a firm's employment. This implies that our dependent variable--change in employment at a firm--should respond to changes in the number of subsidized workers available to and hired by the firm. Measuring this change in subsidized hiring was not easy, however, for the level of subsidized employment was asked for only the most recent year of participation. The procedure for measuring change in the face of this difficulty is described in Appendix B.

The empirical analysis of the employment impact was conducted on a sample of firms, which were divided into size classes. There were several reasons for this. First, the earlier results indicated that smaller firms were less likely to be aware of a given subsidy program and less likely to participate when they were aware. The lack of awareness on the part of small firms suggests that the impact of the program on small firms is of particular interest. Because such a relatively high proportion of these firms did not know about the programs, a large potential employment impact in this segment of the business community could be lying dormant.

A second reason for dividing the sample into size classes is the expected relationship between the standard error from the regression and the size of the firm. The variance of the percentage change in employment should be greater for small, particularly, very small, firms. It is unlikely that this relationship is smoothly related to firm size, however. Preliminary regressions using the entire sample of firms yielded only weak evidence of a relationship between the variance of the error and a variety of transformations of size.

Tables 4.2, and 4.3 present preliminary results of regressions of employment growth between July and December 1979 on a vector of firm characteristics, including the variables for the level and the change in the number of subsidized new hires. The overall sample was that subset of the firms in the survey which answered all of the relevant questions. The firms were then segregated into size classes as indicated. Definitions of the variables used are provided in Appendix B. Because the growth of employment is expected to affect the hiring of subsidized workers, however, it is necessary to eliminate the endogeneity of the usage variables by estimating the model using two stage least squares.

Table 4.2 presents preliminary results that compare OLS and 2SLS impact coefficients for three different models. Model 1 specifies the July-December change in employment as responding to the level of subsidized employment in 1979. Model 2 specifies it to be a function of our estimate of the growth of subsidized hiring in the firm 1979 over 1978. Model 3 enters both variables into the equation. Models 2 and 3 are our preferred models. Since the dependent variable (change in employment) and the subsidy variable are normalized by the same variable (the average of July and December employment), the coefficients reported in table 4.2 can be interpreted as estimates of the net employment ratio--the ratio of induced change in employment to total subsidized employment. These coefficients are downward-biased estimates of this ratio, however. The key subsidy variable is year-to-year growth of subsidy usage, 1979 minus 1978. The dependent variable is change in employment over a 5.3 month period at the end of 1979. Part of the response to the year-to-year growth of the subsidy programs might already have taken place by July 1, 1979, and therefore would not be counted in our impact estimates. In the OLS equations, errors in measurement of the change in subsidized worker variable are a further source of downward bias in our estimates.

With these caveats in mind, let us examine the results. Fifteen of the sixteen coefficients on change in subsidized employment are positive. Ten of

TABLE 4.2

IMPACTS OF SUBSIDY PROGRAMS ON
JULY-DECEMBER CHANGES IN EMPLOYMENT

	Establishment Size			
	1-5	6-20	21-100	100+
Ordinary Least Square Models				
Model 1--% Subsidized	-.05 (1.09)	.03 (.26)	.31* (1.69)	.27 (.38)
Model 2--Change % Subsidized	.01 (.33)	.02 (.21)	.64**(2.33)	.24 (.34)
Model 3--% Subsidized	-.82 (1.54)	.03 (.15)	.03 (.10)	.24 (.18)
Change % Subsidized	.05 (1.13)	.01 (.03)	.67 (1.61)	-.01 (.01)
Two Stage Least Square Models				
Model 1--% Subsidized	-.13 (.79)	.16 (.51)	.91 (1.21)	.23 (.19)
Model 2--Change % Subsidized	.20 (1.34)	.05 (.58)	1.34 (.61)	.81 (.36)
Model 3--% Subsidized	-.10 (.65)	.18 (.56)	.85 (1.09)	-.58 (.24)
Change % Subsidized	.19 (1.27)	.14 (.23)	.76 (.33)	1.74 (.39)
Number of Establishments	1345	1578	1228	472

t statistics are in parentheses.

* significant at .05 level on one tail test.

** significant at .025 level on one tail test.

the sixteen coefficients on 1979 levels of subsidized employment are positive. Only two coefficients, however, are statistically significant at the 5 percent level or better on a one tail test. The estimated coefficients suggest that establishments with fewer than twenty employees are less responsive than larger establishment. In the preferred (#2) 2SLS model, the impact coefficient is .2 for establishments with five or fewer employees and .048 for those with six to twenty employees. The contrast with the larger companies is dramatic where impact coefficients are 1.34 for establishments with twenty to one hundred employees and .81 where employment is greater than one hundred. Unfortunately, the 2SLS results are rather imprecise, particularly for larger establishments. In the small establishments, we may reject the hypothesis that the net employment ratio is one or greater, but we cannot reject the hypothesis that it is zero or negative. For the larger establishments, neither of these hypotheses can be rejected.

TABLE 4.3

2SLS REGRESSIONS PREDICTING PERCENT CHANGE IN EMPLOYMENT AS A FUNCTION OF CHANGE IN SUBSIDY

	Employees							
	1-5[a]		6-20		21-100		100+	
Change % Subsidized	.274	(1.11)	.048	(.08)	1.34	(.61)	.811	(.36)
Log Establishment Size	- .004	(.27)	-.913	(.46)	2.87	(1.63)	- 1.567	(.91)
Unionized Establishments	.04	(.20)	-4.10	(1.18)	.05	(.02)	6.70	(1.87)
Ind. Emp. Growth Rate 1979	.0026	(.98)	.008	(.12)	- .031	(.53)	- .033	(.31)
Log Labor Market Size	.003	(.04)	-.407	(.21)	1.24	(.56)	7.759	(2.25)
Unemployment Rate 1979	- .041	(1.20)	-.129	(.14)	- 1.23	(1.22)	.733	(.46)
Industry Variables								
Construction	- .387	(2.46)	- 7.399	(2.07)	-12.7	(3.40)	.617	(.11)
Finance Services	- .100	(.79)	5.13	(1.73)	.228	(.08)	4.880	(1.36)
Wholesale, Retail	- .118	(.95)	4.54	(1.65)	2.41	(.88)	5.426	(1.49)
Transportation, Communication	-2.32	(1.18)	9.76	(2.11)	5.0	(1.12)	- 2.129	(.37)
Motor Vehicle Sales	.121	(.51)	- .178	(.03)	- 7.6	(1.90)		
Hotels	- .847	(3.72)	-14.68	(2.33)	-14.5	(2.68)	- 9.168	(1.04)
Amusement	.880	(2.57)	2.67	(.41)	-11.07	(1.49)		
Oil, Gas Production	1.347	(2.57)	.05	(.00)	- 3.13	(.37)	9.341	(.83)
Lumber	- .431	(2.61)	11.21	(3.07)	- 4.15	(.92)	-11.060	(1.45)
Location Variables								
Gulf	.101	(.87)	3.03	(1.02)	- .09	(.03)	12.97	(2.80)
San Antonio	.187	(1.12)	9.63	(2.51)	- 2.3	(.58)	- .220	(.04)
Washington	.220	(.90)	3.43	(.51)	7.1	(.92)	10.567	(.88)
Appalachia	.382	(1.50)	- .39	(.05)	7.04	(.87)	26.241	(2.09)
Colorado	.047	(.19)	.545	(.08)	- 1.79	(.24)	- .546	(.04)
Wisconsin	.112	(.53)	- 6.64	(1.16)	5.47	(.86)	22.79	(2.24)
Missouri	- .004	(.02)	5.75	(.76)	- 1.02	(.12)	21.173	(1.63)
Birmingham	.093	(.58)	7.61	(1.91)	.90	(.24)	- 2.183	(.37)
γ	.007		.60		.21		.80	
R^2	.038		.046		.039		.078	
σ_e	1.01		29.3		27.2		26.02	

[a]The 1-5 employee firm models predict the absolute change in employment.

4.4 Employment Change in 1980

In this subsection we examine the impact of knowledge and previous use of targeted employment subsidies on employment change between the December 1979 reference date and the date of the interview. Most of the interviews were completed in April or May of 1980. No data were collected on whether the firm

hired subsidy-eligible workers in 1980 or how many such workers they hired in 1980. Consequently, the models to be estimated address different issues than those examined in the previous section. The issues to be addressed are (a) what is the impact of knowledge of the existence of these programs on employment growth and (b) what is the impact of 1979 usage of these subsidy programs on the growth of employment.

Controlling on the previous use of these programs, we would expect familiarity with the programs to tend to increase the establishment's growth. The impact of previous use of these programs is captured by entering four separate variables into the model: used TJTC in 1979, used WIN in 1979, used CETA-OJT in 1979, and total subsidized employment in 1979. Certainly the 1979 users of the subsidies are more likely than other firms to use subsidies again in 1980. Since employment growth is the dependent variable, the important issue is whether they are likely to increase their level of usage still further. The principle of regression toward the mean predicts that they will become smaller users over time and that, therefore, the impact of previous use variables should be negative. There are, however, special circumstances that may operate in the opposite direction. Tax credit eligible employees hired in 1979 who remained with the firm in 1980 made their employer eligible for a larger tax credit. The TJTC program began in the summer of 1979 and was growing throughout the period. It may be that the 1979 users of TJTC were destined to become even bigger users in 1980.

The coefficients on the relevant variables are presented in table 4.4. The hypothesis that the inclusion of the eight variables describing the subsidy programs did not contribute to explanatory power of the regression was tested using an F test. This hypothesis was rejected for establishments with five or fewer employees and for establishments with six to twenty employees. The small sample of large (100+ employees) establishments makes the results for this group unreliable so we will focus on the results for the other employers. The number of subsidized employees in 1979 had a consistently negative (two of three significantly so) impact on the 1980 rate of employment growth.

The coefficients on the dummy for 1979 usage of TJTC were positive, and two of them were significant. In the stratum for establishments with six to twenty employees, the coefficients on 1979 WIN and CETA usage dummies were positive and statistically significant. In the other strata these coefficients were close to zero. These results suggest that the firms that were the big users of the programs in 1979 became smaller users in 1980, and therefore, contracted in size. They also suggest that the token participants of 1979 tended to become bigger users in 1980 and, therefore, grew more rapidly than firms that were and remained unaware of the program. The majority of the coefficients for the knowledge dummies are positive as predicted but none are significant.

Toward the bottom of table 4.4 we present summary calculations of the impact of previous use and knowledge of these programs on employment growth during the first six months of 1980. The percent of 1979 employment that was subsidized is given in row 1. Row 2 presents the summed impact of the four

TABLE 4.4

IMPACT OF 1979 TARGETED SUBSIDY USE AND KNOWLEDGE ON
PERCENT EMPLOYMENT GROWTH AFTER DECEMBER 1979

	Establishment Size			
	1-5	6-20	21-100	100+
Previous Use Variable				
Used TJTC in 1979	.84 (.22)	18.70 (2.28)	10.00 (1.54)	.50 (.10)
Used WIN in 1979	- .21 (.40)	32.00 (3.03)	- 1.00 (.16)	- 4.60 (.72)
Used CETA in 1979	- .06 (.29)	9.30 (1.89)	.50 (.13)	.10 (.02)
Total Subsidized Employment in 1979	- .13 (1.20)	- .63 (3.27)	- .39 (1.78)	.39 (.57)
Knowledge Variables				
Knew of TJTC in Sept. 1979	.20 (1.64)	- 3.80 (1.24)	.70 (.26)	.50 (.18)
Learned of TJTC after Sept. 1979	- .18 (1.47)	- .40 (.13)	2.00 (1.00)	- 4.40 (1.52)
Knew of WIN in Sept. 1979	.03 (.30)	2.00 (.91)	.20 (0.90)	2.55 (1.00)
Learned of WIN after Sept. 1979	.08 (.08)	- 1.31 (.48)	2.90 (1.04)	- 3.40 (.97)
F for Inclusion of Subsidy Program Variables	1.91	2.27	.85	.86
R - Squared	.10	.13	.10	.17
Standard Error of the Regressions	1.06	30.0	27.9	22.8
Number of Observations	1343	1578	1228	472
1. Percent of Total 1979 Employment Subsidized (Mean in Sample)	3.32%	1.30%	.68%	.45%
2. 1980 Impact of Previous Use	- .38%	.14%	.08%	.049%
3. 1980 Impact of Knowledge	.51%	- .10%	.64%	- .045%
4. Total 1980 Impact	.14%	.04%	.56%	.004%
5. Ratio of 1980 Induced Growth to Subsidized Employment in 1979	.04	.03	.82	.01

t ratios are in parenthesis to the right of the coefficient.

previous use variables on 1980 growth. Row 3 presents the summed impact of the four knowledge variables and row 4 the impact of both previous use and knowledge together.

They are obtained by multiplying the means of each variable by its coefficient and adding. For example, because some firms had obtained subsidies in 1979, the average growth rate of all of the firms with one to five employees in our sample was .38 of a percentage point lower than it would otherwise have been. (In other words, because of regression to the mean, some of the firms that expanded in 1979 because of the subsidy contracted in 1980.) We also calculate that because the firms that knew about the subsidy programs seem to have grown more rapidly as a result, the average growth rate of all firms in this class was .51 of a percentage point higher. The net impact of both together is a small, .14 of a percentage point, increase in average employment. An impression of how small is obtained by comparing these estimates of percentage changes in employment because of previous use and knowledge to the level of subsidy in 1979, 3.32 percent (see row 1). The ratio is .04. The ratio of subsidy induced 1980 employment growth to 1979 subsidy receipt is also close to zero for the companies with six to twenty employees and over one hundred employees. The most notable thing about these estimates is not their small size but the fact they are not negative. They imply that any gains in employment induced by 1979 participation in subsidy programs were maintained during the first five months of 1980.

The net impact of previous use and knowledge together is of significant size only in the twenty-one to one hundred employee company stratum. The knowledge coefficients are positive as hypothesized. The coefficients imply that learning about TJTC after September 1979 increased summer 1980 employment by 2 percent and learning about WIN after September increased summer 1980 employment by 2.9 percent. These effects outweigh negative effect of previous use. For companies with twenty-one to one hundred employees the combined impact of previous use and knowledge seems to have been to increase employment in the sample by roughly .5 percent.

4.5 Summary

This chapter has examined the available evidence on the impact of the targeted private sector employment subsidy programs, TJTC, WIN, and CETA-OJT, on the total employment of participating firms. We found that when employers were asked what impact these programs had upon their employment levels, 25 percent of our respondents reported that they had increased employment. The size of the reported employment increase was roughly one-fifth of the number of subsidized workers hired.

The econometric estimates of the impact of these subsidy programs are even more favorable. About three quarters of all private wage and salary employment is in establishments with twenty or more employees. Only a slightly smaller proportion (70 percent) of all subsidized employees work in establishments that have more than twenty employees. In these establishments our preferred model 2 (the 2SLS change in percent subsidized model) obtains point

estimates of the impact of hiring subsidized workers that imply that extra subsidized employees produce an approximately one for one increase in the establishment's total employment. The ordinary least squares estimates of the same model also imply a large impact: the change in employment per subsidized worker was .64 for establishments with twenty-one to one hundred employees and .235 for establishments of greater than one hundred employees.

These OLS and 2SLS results are our best point estimates of the impact of the subsidy programs on the employment levels of the subsidized firms. We have argued that if anything they are biased down because they cover only the final five and a half months of 1979. The analysis of 1980 employment growth suggests that there was no tendency for these 1979 gains to be lost in 1980. The problem with these results, however, is that the confidence intervals (especially those that use two stage least squares) of these point estimates are very wide. In the 2SLS models the hypothesis of no subsidy program impact cannot be rejected. In the OLS models we may reject the hypothesis of no impact only for the twenty-one to one hundred employee establishments. As a result we cannot draw conclusions from this analysis with any great confidence. Unless the true impacts had been unreasonably large, there was no way our data set with its small number of participating firms could have produced an estimate of subsidy impacts on the employment that was significantly different from zero. All we can say is that the subsidy programs seemed to be having a significant impact on the employment of participating establishments with more than twenty employees in 1979, but that these results could have been a statistical fluke. There are also other reasons for treating these findings with caution. It is the long term impacts of these programs that matter but only short term (5.5 month) impacts were modeled and the major program being examined, TJTC, had only just gotten underway. Finally, the reader is asked to remember the point made in Section 4.1: the general equilibrium impact of these programs can be quite different from their impact on participating firms.

APPENDIX 4A

Derivation of Δ Subsidized Employment

Definition of Δ% SUBSIDIZED

The change in the percentage of a firm's employees who were subsidized under CETA, WIN or TJTC from 1978 to 1979 was estimated in accordance with the following schedule.

1.	0	If the firm hired no subsidized workers in 1978 or 1979
2.	- % Subsidized	If the firm hired in 1978 but not 1979
3.	+ % Subsidized	If the firm hired in 1979 but not 1978
4.	$\frac{.645}{1.645}$ Subsidization	If the firm hired in both 1979 and 1980

The coefficient of % Subsidization under case 4 was calculated by estimating the effect of 1978 participation in one of the programs upon the log of % Subsidization. Using a sample of firms which received a subsidy for a worker hired through WIN and/or CETA in 1979, or through TJTC, the log of the percentage of a firm's (full-time equivalent) labor force which is subsidized was regressed on a vector of firm characteristics and a dummy for firm participation in 1978. The coefficient of the dummy for particiaption in 1978 was .498 implying that the expectation of the log of % Subsidized Hiring, conditional on X, was 65% larger if the firm had hired subsidized workers in the previous year. Therefore the increase in the number of subsidized workers, % SUBSIDIZED (79) - % SUBSIDIZED (78), was equal to

$$\left(1 - \frac{1}{1.645} \times \%\ \text{SUBSIDIZED (79)}\right), \text{ or } \frac{.645}{1.645} \times (\%\ \text{SUBSIDIZED (79)}).$$

Estimation of the Coefficient on Participation in 1978

In calculating the increase in subsidized hiring, we have predicted the impact of participation in one year upon the level of hiring in the following year for those firms, and only those firms, who participated in the former period. When attempting to identify this relationship, we need to allow for the fact that censorship of a random sample of firms into a subsample of those who participated in the programs in question may introduce a sample-selection bias.

The censored sample problem is well documented in the econometric literature. Following Heckman (1976), suppose that it is our intention to relate subsidized hiring, S_i, for the i^{th} firm, to a vector of firm characteristics X_i. That is, assuming a linear relationship, we want to find the B vector that satisfies

(A.1) $S_i = X_i B + U_i$

for all i such that $S_i > 0$ (where all notation is standard). If the relationship between S_i and X_i is as in (A.1) we can only observe S_i for those firms such that $S_i > \underline{0}$. The population regression of (A.1) is

(A.2) $E(S_i|X_i) = X_i B$

Our interest is in estimating

(A.3) $E(S_i|X_i, S_i>0) = X_i B + E(U_i|S_i>0)$

This appears to be the truncated dependent variable problem described by Tobin (1958), in which the expectation of the error cannot, in general, be assumed to equal zero. If, however, there are characteristics of the firm which influence whether or not the firm participated but not the level of hiring given participation, it is more useful to view our problem as consisting of two aspects of firm behavior: (a) the participation decision, and (b) hiring conditional on participation. We might, for example, let Y_i be (to use the standard metaphor) the "intensity" of the firm's desire to participate in the government program in question. We observe S_i only when Y_i crosses some threshold; assumed without loss of generality to be zero. The formulation of the problem then becomes one in which (changing notation slightly)

(A.4) $S_i = X_{1i} B_1 + U_{1i}$

(A.5) $Y_i = Y_{2i} B_2 + U_{2i}$

We hope to estimate the regression formulation

$E(S_i|X_i, Y_i>0) = X_{1i} B_1 + E(U_1|U_2>0)$

or

(A.6) $E(S_i|X_i, Y_i>0) = X_{1i} B_1 + E(U_{1i}|U_{2i}>-X_{2i} B_2)$

In general the covariance between U_1 and U_2 will be nonzero (in the simple truncation case U_1 and U_2 are identical) and the second term on the right hand side of (A.6) will not be zero. Furthermore, if X_2 and X_1 are related (and X_2 almost certainly contains elements of X_1 in our case) the error term associated with a simple regression of S_i on X_{1i} for all participating firms will be correlated with X_1. The coefficients of X_1 resulting from that regression experiment will, therefore, be biased. Heckman (1980), shows that this can be viewed as an omitted variable problem which could be solved if we could estimate the conditional mean of U_1 in (A.6).

It has been shown in the literature (Heckman (1976), that the regression function needed to estimate A.6 is actually

(A.7) $E(S_i|X_i, Y_i>0) = X_{1i} B_1 + \frac{\sigma_{12}}{\sqrt{\sigma_{22}}} i + V_{1i}$

where σ_{12} is the covariance of U_1 and U_2, σ_{22} is the variance of U_2 and $_i$ is a correction term such that

$$(A.8) \quad \lambda i = \frac{f(-X_{2i}B_2/\sqrt{\sigma_{22}})}{1 - F(-X_{2i}B_2/\sqrt{\sigma_{22}})}$$

where f(.) and F(.) are the probability density function and the cumulative distribution function, respectively, of the standard normal distribution. The error term V_{1i} in equation (A.7) has the properties

$$E(V_{1i}) = 0 \quad \text{and}$$

$$E(V_{1i}^2) = \sigma_{11} \left[(1-\rho^2) + \rho^2\left(1 + \frac{-X_{2i}B_2\lambda_i}{\sqrt{\sigma_{22}}} - \lambda_i^2\right)\right]$$

where $= \frac{\sigma_{12}}{\sqrt{\sigma_{11}\sigma_{22}^2}}$

It can be seen that the denominator of (A.8) is the probability that $Y_i > 0$; that is, the probability that the i^{th} firm has participated in a subsidy program. It is possible to estimate $\frac{-X_{2i}B_2}{\sqrt{\sigma_{22}}}$ and thus λ_i, using a probit model of firm participation in the programs. The estimated λ_i term can be substituted into equation (A.7), which is then estimated by regression.

In this paper a maximum-likelihood probit model was used to estimate λ_i for each firm in the sample which received a subsidy for at least one worker in 1979. The correction term was then substituted into an equation which predicted subsidized hiring for those firms participating in at least one of the programs. The results from this are reported in Table A-1 equation 1.

Table B-1 reports the OLS results from the regression of the log of the percent subsidization on a vector of firm characteristics. Because the expected value of $(V_i)^2$ in equation (A.7) is not constant, however, it is necessary to correct for heteroskedacity. This was accomplished, as Heckman recommends, by regressing the residuals from A.7 on a constant term and the variable $\left(\frac{-X_{1i}B_1\lambda i - \lambda_i^2}{\sqrt{\sigma_{22}}}\right)$. to generate the weights for a weighted least-squares regression. A regression of the squared residuals from the equation in Table A-1 on this variable proved, however, to have extremely low explanatory power. (Also because a true λ_i cannot be observed, which makes the standard errors incorrect.) Therefore, the ols estimate was used to calculate the change in subsidized employment.

TABLE 4.5

Dependent Variable ln(# Subsidized Employees/# FTE Workers)

Model

Independent Variable	OLS
Proportion Minority	.014 (1.31)
Proportion Disadvantaged	- .011 (- .59)
Log Labor Market Size	- .194 (.118)
Unemployed	.043 (.553)
% Employed	.055 (210.)
Expected Growth	.280 (4.41)
Expected Dummy	.326 (1.02)
% Part-time	- .094 (- .25)
% Low Skill	- .125 (- .52)
Unionized Establishment	.87 (- 3.27)
Mining and Manufacturing	- .498 (- 1.70)
Wholesale and Retail	.37 (- 1.32)
Finance and Services	- .066 (- .232)
Transportation and Communication	.222 (- .55)
Reliable Unskilled Workers Not Available	.187 (1.34)
Midwest	- .383 (- .86)
Southeast	- .389 (- 1.29)
West	- .327 (- .57)
Log Establishment Size	- .0006 (- 3.52)
Firing Flex	- .703 (- 4.86)
40 Hours Full-time	- .255 (- 1.56)
Participated in 1978	.498 (3.39)
	.080 (.58)
Constant	4.15 (2.31)

APPENDIX 4B

Definitions of Variables

1.	UnempRate 79:	The unemployment rate at the site in 1979.
2.	IndEmpCHG 79-80:	The % change in employment at the national 3-digit level of the relevant industry from January 1979 - January 1980.
3.	Log Labor Market Size:	Logarithm of the number of employed persons in the site. A proxy for the size of the local labor market.
4.	Unionized Establishments:	The percentage of the firm's labor force covered by collective bargaining agreements.
5.	ΔDec - July:	200* (December - July employment)/(December employment + July employment)
6.	ΔCurrent - July:	200* (current employment - July employment)/(current employment + July employment)
7.		

Industry Dummies

8.	MIN-MAN:	1 if the firm was engaged in manufacturing or mining, 0 otherwise.
9.	TRN-COM:	1 if the firm was in transportation or communication.
10.	WHOL-RET:	1 if the firm was engaged in wholesale or retail trade, 0 otherwise.
11.	FIN-SERV:	1 if the firm was in finance or services, 0 otherwise.
12.	OIL-GAS:	1 if the firm was engaged in the extraction or refinement of oil or natural gas, 0 otherwise.
13.	LUMBER:	1 if the firm was engaged in the production or sale of lumber or lumber products, 0 otherwise.
14.	MVManu:	1 if the firm manufactured motor vehicles, 0 otherwise.
15.	MV Sales:	1 if the firm was engaged in motor vehicle wholesale or retail trade.
16.	HOTELS:	1 if the firm was a hotel or motel, 0 otherwise.

17. AMUSE: 1 if the firm was classified as providing amusements, or was a movie or other kind of theater, 0 otherwise.

Location Dummies

18. WISCONSIN: 1 if the firm was in one of three Wisconsin sites, 0 otherwise. (See list of sites in Appendix 18.)

19. Gulf: 1 if the firm was located along the Gulf coast (in Florida, Lousiana, Alabama, or Texas; see list of sites).

20. EASTERN: 1 if the firm was located in 4 counties in Kentucky and Virginia in which coalmining is dominant, 0 otherwise.

21. SAN ANTONIO: 1 if the firm was located in San Antonio, Texas.

22. COLORADO: 1 if the firm was in one of three Colorado sites, 0 otherwise.

23. BIRMINGHAM: 1 if the firm was located in Birmingham.

24. MISSOURI: 1 if the firm was in one of three Missouri sites, 0 otherwise.

25. WASHINGTON: 1 if the firm was in one of three sites in western Washington state, 0 otherwise.

Previous Use of Subsidy Program Variables

26. WIN 78: 1 if the firm received a WIN subsidy in 1978, 0 otherwise.

27. CETA 78: 1 if the firm received a CETA subsidy in 1978, 0 otherwise.

28. NJTC: 1 if the firm received a subsidy through NJTC, 0 otherwise.

29. NJTC Frust.: 1 if the firm claimed to have increased employment as a result of NJTC but failed to receive a subsidy through that program.

30. Paper Work: How many employee-hours that the firm expected to be required to apply for a CETA subsidy. 0 if the firm did not have any expectation (didn't know).

31. Paper Work D.K.: 1 if the firm answered "didn't know" how many hours a CETA application would take.

CHAPTER 5

THE DESIGN OF EMPLOYMENT SUBSIDIES: THE LESSONS OF THE UNITED STATES EXPERIENCE

John H. Bishop

The industrialized West is finding it increasingly difficult to reconcile the twin objectives of low rates of inflation and low rates of unemployment. Monetary and fiscal policies seem to be unable to reduce unemployment below unacceptably high levels without accelerating an already unacceptably high rate of inflation.

Private sector employment and training subsidies have been proposed as a potentially effective mechanism for dealing with this problem. They lower the cost of labor and the marginal cost of extra output and thus they should simultaneously increase employment and lower prices. The purpose of this chapter ter is (a) to summarize what has been learned about the effectiveness of employment subsidies as a remedy for stagflation and (b) to examine, in the context of this objective, how they are best designed. This chapter reviews all of the relevant research, not just the material presented in previous chapters.

The United States experience with New Jobs Tax Credits (NJTC), the WIN Tax Credit, the Targeted Jobs Tax Credit (TJTC), and CETA on-the-job training subsidies yields a number of important lessons about how employment subsidies should be designed. The main body of the chapter describes these programs, summarizes the studies that have been done of their effectiveness, and explores how the design characteristics of each program influenced its success or failure. The most successful of the United States employment subsidies, the New Jobs Tax Credit, is discussed in the first section. The more targeted programs are discussed in section two. The final section of the chapter applies the lessons developed earlier to the design of a prototype employment subsidy.

There are at least five questions we would like our review of each of these programs to answer:

1. Is there significant employer participation?

2. Does the subsidy induce the employers who receive it to increase employment of workers eligible for subsidy?

3. Does total employment in the industry or nation increase? To what extent does the subsidy of certain workers and firms cause unsubsidized workers and firms to suffer declines in employment?

4. Does the subsidy produce a reduction in product prices?

5. Do subsidized workers get higher wages and does this tend to promote inflationary wage increases?

The first question is the easiest to study. If a program does not succeed in attracting significant employer interest (this can be either many employers using the subsidy intermittently or a few employers using it extensively), it cannot make a major contribution to reducing stagflation. (It might, however, be effective in helping a limited number of disadvantaged workers.) The second question can be addressed by comparing employers who have used the subsidy to other similar employers who have not. A finding that the subsidy causes recipient firms to expand their employment of workers eligible for subsidy does not necessarily imply that total employment is rising, however. It may only be that eligible workers are being redistributed among firms. The other three questions are the hardest ones to answer. Answers are necessary, however, before a definite judgment can be made that employment subsidies are an effective way of dealing with stagflation. With one exception, the empirical work on the United States experience does not address the last three questions. The burgeoning theoretical literature on employment subsidies has addressed these questions, so this literature is referred to where appropriate.

5.1 The United States Experience with the New Jobs Tax Credit

Signed into law in May 1977, the New Jobs Tax Credit (NJTC) offered firms a tax credit against corporate or personal income tax liability for expansion in employment in 1977 or 1978. NJTC was a marginal subsidy of 50 percent of the increase in an employer's wage base under the Federal Unemployment Tax Act above 102 percent of the previous year. The scheme was only mildly targeted but simple enough in administration for employers to calculate their own eligibility on their tax forms.

Despite the fact that public agencies made very little effort to advertise or promote the NJTC and that many small firms had not yet heard of it in February 1978, use of NJTC was quite extensive. At the end of 1977, its first year of operation, $2.3 billion of New Jobs Tax Credits were claimed on a total of 610,000 tax returns. In 1978 $4.4 billion of credits were claimed on a total of 1,107,000 tax returns. In 1979, $1.6 billion were claimed on 300,000 returns with tax credits carry-overs or with fiscal years that overlapped with 1978. Since the firms' deductions for wages must be reduced by the amount of the credit, revenue costs (assuming no direct effects on before tax profits) were approximately $5.0 billion. While roughly one-third of the returns claiming a credit were corporate returns, two-thirds of the dollars claimed were on these returns. Since the credits due to a partnership or Subchapter S corporation may show up on more than one individual return, the total number of businesses claiming the credit is smaller than the number of tax returns claiming it. Nevertheless, more than 30 percent of the nation's 3.5 million employers claimed the credit in 1978. A lower bound estimate of the number of workers whose employers received subsidy can be obtained by dividing the dollars of credit claimed by $2,100, the maximum credit an employer can receive for one worker. This calculation implies that during its two year period of operation, at least 3.95 million employees were subsidized. By comparison, total private nonagricultural employment grew 2.8 million in 1977 and 3.6 million in 1978.

5.1.1 The Impact of NJTC on Subsidized Firms

To date, there have been three studies of the New Jobs Tax Credit. Two have focused on the differential impact of NJTC on the firms that knew about it or said they responded. The other studied the credit's impact on total employment of entire industries and on inflation.

The first study is based on a mail questionnaire survey of a sample of the membership of the National Federation for Independent Businesses (NFIB) (McKevitt 1978). The first survey to ask questions about NJTC was conducted in January 1978. Of the employers responding, 43 percent knew about NJTC and 1.4 percent reported that the credit had influenced them to hire extra workers (the number averaged 2.0 per firm). The April survey found that 51 percent knew of NJTC's existence and that 2.4 percent had increased hiring by an average of 2.3 employees as a result. In the July 1978 survey, 58 percent were aware of the credit, and 4.1 percent of the firms reported they had increased hiring as a result. An increase in employment of 2.3 employees by over 4 percent of all employers is not a small response. If the NFIB survey is representative, and other firms are not hurt by the expansion of subsidized firms, these responses imply that in the second quarter of 1978 there were more than 300,000 extra jobs directly created as a result of the NJTC at a tax expenditure of roughly $6,500 for each job created.[1]

The second study (Perloff and Wachter 1980) is based upon a survey conducted by the United States Bureau of the Census. Perloff and Wachter compared rates of employment growth between 1976 and 1977 for firms that knew about the credit and those that did not. Holding employment size, class, region, form of organization, type of industry, and the growth rate of sales constant, they found that the employment of firms that had heard of the credit before February 1978 had grown 3 percent faster. Firms that reported they made a conscious effort to expand employment because of the credit grew 10 percent faster than firms that knew about the credit but did not report making any special effort. Since firms may learn about the credit because they are growing fast, Perloff and Wachter conducted a Wu test for simultaneity and found themselves unable to reject the hypothesis that knowledge of the credit was uncorrelated with the error term of the employment change regression. If one were to assume that NJTC caused the 3 percent higher growth of the small and medium-sized firms that knew about the credit (about a quarter of total employment was in these firms) and that the NJTC left the rest of the economy unaffected, the total number of extra jobs in 1977 would have been roughly 700,000. Tax expenditure per job created would have been $2,000 per job.[2]

5.1.2 The Impact of NJTC on Total Employment

Studies like those just reviewed are measuring the differential impact of NJTC across firms, not the net impact of NJTC on a total economy. Since firms compete with each other in both labor and product markets, the increases of employment in subsidized firms may cause decreases of employment for their unsubsidized competitors. Alternatively, an NJTC-induced expansion by one firm may cause that firm's suppliers to expand as well. The direction of

NJTC's impact on nonsubsidized firms is uncertain, because it depends upon the relative size of offsetting effects. We suspect, however, that the first effect is larger than the second. If so, simple extrapolations from the measured impact of the credit on firms to impacts on the economy like those in previous paragraphs will exaggerate the true impact. Since most of the displacement effects that may bias estimates of net job creation when the firms are the unit of observation are netted out when the industry is the unit of observation, studies of NJTC's impact on an entire industry's employment would seem to be able to resolve this issue.

The third study (Bishop 1981) attempted to address the displacement and inflation effects of the NJTC by examining its impact on two of the industries--construction and distribution--in which one would expect the largest response. Nonseasonally adjusted monthly data on employment and total hours worked in these industries were regressed on seasonal dummies, trends on the dummies, and three-year distributed lags on input prices and retail sales (or construction put in place). With few exceptions, the lag structures were freely estimated, with each input price or price ratio being represented by its contemporaneous value, and by that of each of the previous four quarters and four half-years. Models were estimated using both ordinary least squares and two stage least squares.

The NJTC variable was an average (over the past six months) of the proportion of firms (weighted by employees) that knew about the credit. It had a value of .057 in June 1977 and rose at an average rate of .0424 per month, reaching .343 in January 1978 and .572 in June 1978. Most of the coefficients on the NJTC variable were positive and significant. Across all of the regressions the average NJTC employment stimulus over the twelve-month period from mid-1977 to mid-1978 ranged from 150,000 to 670,000 depending on specification. Hours worked per week declined, so total hours worked rose proportionationately less.

5.1.3 The NJTC's Impact on Price Inflation

Employment subsidies lower both the average and marginal costs of production and sales. In competitive industries, output will expand and prices will fall. Given the wage level, supply curves of most competitive industries are quite flat, so if the subsidy does not raise wage rates, the price reduction should be nearly as large as the downward shift of the supply curve. A marginal employment subsidy such as the NJTC lowers the marginal costs of existing firms and the marginal and average costs of new firms by a lot more than it lowers the average costs of existing firms. Consequently, a marginal subsidy may induce price reductions in competitive industries that, in the short and medium run, are substantially larger than the total dollar amounts of subsidy paid out.

The unsettled nature of the theory of oligopolistic pricing makes infeasible definitive theoretical predictions of how oligopolistic industries will respond to marginal employment subsidies. Some theories predict a price decline that is equal to the reduction in average costs of existing firms.

If, however, the oligopoly is setting its price just low enough to forestall or limit entry of new competitors into the industry, their response will depend on the subsidy's impact on the average cost of new entrants. Having a zero threshold, a new entrant receives subsidy on all workers. Consequently, a marginal employment subsidy with a fixed threshold that is perceived to be permanent should cause the entry forestalling price to decline by the full amount of its subsidy of marginal costs. A smaller decline will occur if the threshold is revised in the future, if the subsidy is not viewed as permanent, or if, as with the NJTC, limitations are placed on the subsidy that new firms can receive. Nevertheless, here again theory predicts that the short and medium run price decline may be substantially larger than subsidy-induced reduction in the average costs of existing firms.

An examination of the behavior of prices during the NJTC's period of operation tends to support the hypothesis that a marginal employment subsidy can temporarily slow inflation. A number of the features of the NJTC--the $100,000 per firm limitation, subsidizing only the first $4,200 of wages, and its temporary nature--should have focused the subsidy and stimulus to production on sectors of the economy with high rates of employee turnover and large numbers of small and medium-sized firms. The distribution sector--trucking, wholesale, and retailing--fits this description, so it was hypothesized that NJTC would tend to compress the margin between retail prices and manufacturers' prices of finisned comsumer goods. Preliminary support for this hypothesis is provided by the fact that between May 1977 and June 1978 nonfood commodity retail prices rose only 4.73 percent, while manufacturers' prices of nonfood, consumer finished goods were rising 6.56 percent.

The hypothesis was tested econometricly by regressing the monthly rate of change of retail prices on current and lagged changes in a number of distribution industry cost variables--wage rates, wholesale prices, the rental price of capital, and excise taxes--the unemployment rate, seasonal dummies, and trends on the seasonal dummies. Coefficients on NJTC were negative and statistically significant for the nonfood commodities aggregate and for restaurant and tavern prices. The size and statistical significance of the NJTC coefficients were robust for a variety of changes of specification. The five to seven percent reduction in marginal costs induced by the credit seems by June 1978 to have lowered prices for nonfood commodities by nearly 2 percentage points and for all commodities by roughly 1 percent.

The savings to the consumer from the compression of retail margins seems on its own to have been roughly comparable to the face value of tax credit claims. Tax credit claims in 1977 were $2.4 billion; the coefficients imply consumer savings were between $.5 and $1 billion. In 1978, claims were $4.5 billion and consumer savings were estimated to be between $3.8 and $7 billion. If the prices of services and some manufacturing goods were forced down as well, the NJTC-induced price reduction during its second year could well have been two, three, or four times the size of the tax subsidy. While this result is predicted by theory, it is nevertheless quite remarkable that a marginal employment subsidy handicapped by a two-year life and limited to $100,000 per firm should have had so large an effect.

5.1.4 The NJTC's Impact on Wage Inflation

The primary concern economists have had about marginal employment subsidies is that when labor markets are tight the subsidy may tend to accelerate the rate of wage increase and thus raise the underlying rate of inflation. When it was initiated, NJTC was seen as a temporary countercyclical employment stimulus. It was phased out on schedule in December 1978, primarily because the economy was perceived to be already at the point where further reductions in unemployment rates would result in accelerating wage increases.

Whether employment subsidies do in fact have an impact on wage rates has not yet been formally tested. In table 5.1, the annual rates of change of wage rates for a variety of industries are tabulated for the period May 1975 through April 1980. Wage rates rose more rapidly during the phase-in period of the NJTC (May 1977 through December 1978) than in the previous years. During this period, however, unemployment rates were lower than previously, and the minimum wage was rising more rapidly, so the increase in the rate of wage inflation may have been a response to those phenomena and not a direct response to the NJTC. More to the point, however, is what looks like a deceleration of wage increases in wholesale, retail, construction, and the total private economy during the phase-out period (December 1978 to December 1979) when unemployment remained low. Careful econometrics is required to sort out all these factors but this cursory examination of the evidence

TABLE 5.1

ANNUAL RATE OF CHANGE OF WAGE RATES

			NJTC Phase In		NJTC Phase Out	
	5/75-5/76	5/76-5/77	5/77-5/78	12/77-12/78	12/78-12/79	12/79-4/80
Minimum Wage	9.5	0.0	15.2	15.2	9.4	6.9[b]
Wholesale and Retail	6.2	7.5	8.7	9.8	8.7	6.9
Construction	7.0	4.0	6.9	7.6	6.8	5.3
Services	8.2	6.5	7.1	7.5	8.1	7.0
Manufacturing[a]	6.9	9.1	8.2	9.2	9.0	9.7
Total Private	8.0	7.4	8.1	9.2	8.1	6.1
Unemployment Rate of 20-64 Males	6.5	5.8	4.8	4.1	4.1	4.8

[a] Excludes the effects of overtime and interindustry shifts.

[b] Annual rate of change for 12/79-12/80.

certainly provides no evidence that a reduction in unemployment induced by a marginal employment subsidy is less wage inflationary (as distinct from price inflationary) than a reduction in unemployment engineered by other policy interventions. Consequently, it would seem prudent either to avoid operating a marginal employment subsidy during tight labor markets or to redesign the scheme so that wage increases are discouraged at the same time that employment is stimulated. (Such a redesign is described in section 5.3.)

5.2 U.S. Experience with Targeted Programs

The United States has experimented with a variety of targeted employment subsidies. These programs--WIN tax credit, NAB-JOBS contracts, CETA On-the-Job Training subsidies, and the Targeted Jobs Tax Credit--have all been targeted on highly disadvantaged workers and have as a consequence been quite complicated to administer. In each of these programs, certification of a worker's eligibility has required separate application by and certification of both the worker and the employer. While these programs have helped specific individuals find jobs and get off welfare, they have not yet achieved significant scale and consequently have not had an appreciable impact upon the number of people on welfare or the unemployment rate of people in the target group.

5.2.1 NAB-JOBS

The first of the subsidy programs was the National Alliance for Business's JOBS program contract placements effort in which the government issued contracts that reimbursed employers for part of the cost of hiring and training disadvantaged workers. To qualify for the program a worker had to be a high school dropout, less than twenty-two or more than forty-five years old, handicapped, or in a family with below poverty level income. Contract placements grew from 8,400 in fiscal 1967 to 93,000 in fiscal 1971 and declined thereafter. Thus at its peak JOBS contracts were subsidizing only one-tenth of one percent of the nation's workers. Tight budgets were not responsible for the small scale of the program, for the administrators of the program were consistently unable to expend the funds programmed for JOBS contracts. In 1969, for instance, only $49 million of the $210 million programmed for JOBS contracts was expended.

Also significant is the fact that only one-third of the employers that hired JOBS enrollees went to the trouble of establishing a contractual arrangement and thus received a subsidy for what they were doing. This reveals that the problem is not just one of employers being reluctant to hire stigmatized individuals. Many employers seemed to find the delays and red tape of arranging a contract and the potentially greater vulnerability to affirmative action complaints so potentially costly, that they did not apply for the 50 percent subsidy of the first six months of a worker's wages for which they were eligible.

5.2.2 CETA-OJT

With the reorganization of manpower services mandated by the Comprehensive Employment and Training Act of 1973, the JOBS program evolved into what is now called CETA On-the-Job Training contracts. The OJT program has not developed an effective local constituency because many small business people have an ideological aversion to handouts and because the perceived benefits of participating are so small. The prime sponsors that were included in the EOPP Employer Survey seldom wrote contracts for more than one or two workers even when the participating firm was large. Many local prime sponsors choose to allocate their dollars to classroom training rather than OJT. Seldom does a firm receive more than one subsidized worker, and the maximum payment is generally less than 25 percent of a year's wages. Thus, despite congressional mandates to expand the scale of the program, only 1 percent of the nation's employers participated during 1979.

5.2.3 The WIN Tax Credit

For nearly ten years, employers that have hired recipients of Aid to Families with Dependent Children (AFDC) have been eligible for a tax credit. Despite increases in the rate of subsidy from 10 to 20 percent and now to 50 percent and other liberalization of the terms of the subsidy, claims for WIN tax credits have remained at a level of only thirty to forty thousand full-time equivalent workers for a number of years. This implies that less than 5 percent of each year's new WIN registrants, less than 2 percent of adults receiving AFDC benefits, and less than 10 percent of working welfare recipients have been aided by the WIN tax credit. As with JOBS, only a small proportion of the firms that hire WIN-eligible workers applied for the tax credit for which they were eligible. Either the firms did not know they were eligible, or they found the paperwork too burdensome and the benefit too small to warrant applying. Of those firms that received a WIN credit, less than 10 percent attributed their hiring of the WIN enrollee to the credit (Hamermesh 1978).

5.2.4 The Targeted Jobs Tax Credit

Beginning in 1979 employers outside the personal service sector have been able to obtain a tax credit of 50 percent of the first $6,000 of wages per employee for the first year of employment and 25 percent of such wages for the second year of employment for the hiring of certain categories of workers. These included high school students in cooperative education programs, economically disadvantaged youth (eighteen through twenty-four), veterans and ex-convicts, Supplementary Security Income and general assistance recipients, and the handicapped.

It is too early in the life of the program to predict what its eventual scale will be. Already, however, it has surpassed the scale of the JOBS, CETA-OJT, and WIN credit programs. The program started slow; the cumulative

total number of certifications was only 13,677 by the end of July 1979. By fiscal 1981 it had grown to a point where 400,000 workers were being certified per year.

TJTC's greater success at obtaining employer participation has been due to three features:

1. It is an entitlement. Reluctance on the part of local agencies to administer it cannot prevent a persistent employer from obtaining certification of employees that are eligible. In fact, ETA's 1979 Study of Early Implementation of TJTC found "the rather limited vouchering and certification activity that had taken place by then was largely in response to employer and applicant inquiries rather than active promotion by their staff."

2. At least one target group--the Co-op Ed students--was defined by a characteristic that does not carry stigma. For this group, student and employer certification were made into a one-step process and responsibility was centralized in the hands of a person--the high school official responsible for Co-op Ed--who was being judged by school supervisors on the basis of the number of jobs found for the target group. As a result, 45 percent of all jobs certified for TJTC's have been for Co-op Ed students. The 1981 reauthorization of TJTC limited the eligibility of Co-op Ed students to those from disadvantaged families so this comment does not apply to the current TJTC program.

3. Participation in TJTC requires less paperwork than CETA-OJT or the JOBS and early WIN programs did and requires fewer contacts between government agencies and the employer.

Nevertheless, the TJTC is currently helping less than 10 percent of the pool of young people eligible for the program.[3] In contrast, NJTC attracted in its second year the participation of 50 to 70 percent of all eligible firms. There are three basic causes of TJTC's low participation rate:

1. Most job seekers and most employers are not aware or are only vaguely aware of the program. A spring 1980 survey of employers found that only 17 percent of all employers representing establishments responsible for 33 percent of all employment reported being "familiar" with TJTC (EOPP Employer Survey). Firms that reported being familiar with the program often knew very little about it.

2. There is a stigma attached to being a member of most of the TJTC's target groups. Employers perceive the program to be subsidizing people who do not make good workers. This reduces the likelihood that employers will ask CETA or the employment service to refer TJTC-eligible workers to their firm. Furthermore, many applicants feel that telling prospective employers of their eligibility for TJTC may hurt their chances of getting the job.

3. The complicated rules of eligibility means that most employers are unable to identify who is eligible on their own and that government certification of employee eligibility is necessary. This has three disadvantages: (a) it often forces the firm out of its traditional recruitment channels; (b) employers fear that it will introduce red tape into the hiring process or bring about unwelcome government interference (the costs of identifying and certifying who is eligible are thus major deterents to participation); (c) the program's success depends upon cooperation between private business men and government bureaucrats. (Most employers are very wary of government and the attitude of government employees in some parts of the country reinforces their distrust.)

Our findings in chapter 2 suggest that the first problem can be overcome by publicity and aggressive promotion of the program. Much greater efforts are possible in this area, so it is hoped that this problem is temporary.

The other two problems, however, arise from a mismatch between the structure of the employment subsidy scheme and the recruitment processes that predominate in the relevant labor markets. Each month the typical employer is hiring one employee for every ten already on board (Cohen and Schwartz 1979). The probability that a new hire will still be with the firm a few months later is less than 50 percent. As a result, employers try to keep the cost of searching for new employees to a minimum. Studies of how people have obtained their last job find that 35 percent of all jobs were found by applying directly to the firm without suggestions or referrals and that another 26 percent were obtained by applying directly to the firm at the suggestion of a friend or relative (Rosenfeld 1975). Most firms prefer to hire people who are recommended by current employees or who have shown their desire for the job by personally coming to the establishment and applying. Two-thirds of the employers in the EOPP Survey had not listed a job with the employment service in the previous year. As a result, even though 34 percent of all workers had checked with the employment service during their last period of job search, only 5.1 percent had gotten their jobs through an employment service referral. Employers prefer informal recruitment channels because (a) such channels are faster, (b) employers do not become inundated with job applicants who must be interviewed, (c) prescreening is possible so the number of applicants who are turned down is minimized, and (d) they can avoid dealing with government.

This preference acts to limit the market penetration of any program for finding jobs for the disadvantaged that depends upon a labor market intermediary--Job Service, or a CETA subcontractor such as the Urban League. Such programs can overcome their inherent structural weakness only when unusually dedicated and competent people are running the labor market intermediary. With only ordinary leadership, such a program is bound to be only partially successful--helping some of the people who approach the agency for help but failing to reach most of the eligible population. This structural weakness is exacerbated by the adversarial relationship between government and business. An ETA study done during the summer of 1979 found that many employment service and CETA staff "doubt the value of the tax credit in increasing job placement among the targeted groups or in netting hires among them that would not have

taken place anyway" (ETA 1979, p. vii). The comments of staff seem to reflect a lack of desire to help firms receive a tax benefit to which they are entitled unless the firm reciprocates by changing its behavior (something the tax law does not require).

The targeted employment subsidies that preceded TJTC all necessitated agency referrals of eligible job applicants. With TJTC there are two alternate ways of bringing subsidy, employer, and job seeker together. Job seekers may inform employers of their eligibility. This does not now occur to a significant degree because most eligible workers are unaware of TJTC's existence and because most employment service offices do not routinely inform the eligibles that do come to it for assistance that they are eligible. The other barrier to this mechanism becoming important is the reluctance of many job applicants to advertise their TJTC eligibility for fear they will be stigmatized. This reluctance seems to be justified. An experiment in which TJTC-eligible job seekers were trained to inform employers of their eligibility for a tax credit found that such training caused a statistically significant reduction in placement rates (Burtless and Cheston 1981).

The second alternative mechanism assigns the initiative to the one who most directly benefits from the tax credit, the employer. This scenario envisions employers' screening their job applications for eligible individuals and then sending them down to the employment service for vouchering and certification before or after they are hired. Presumably, anticipating that A may be eligible for subsidy and B is not will increase the probability that A is offered the job. The use of family income and participation in welfare programs as targeting criteria, however, makes it difficult for employers to know who is eligible and thus prevents many employers from taking the tax credit into account when hiring. Sending job applicants over to the employment service prior to hiring does not seem to have become popular for it delays the hiring process, risks losing the worker altogether, and is thought to be unethical by many employers.

Identification of eligibles by the employer (or his agent) seems to have become the primary mechanism by which employers identify and certify TJTC eligible workers. For the first two and a half years of the TJTC program employers could apply for certification of an eligible employee long after the hiring date. The consequence was that many tax credits were awarded for employees whose eligibility was not learned of until after the date of hiring. During this period approximately 63 percent of the non Co-op Ed certifications of eligibility were being obtained after the individual had been hired. This has been interpreted as implying that the tax credit was not influencing many of the hiring decisions that resulted in receipt of a tax credit, and therefore, was producing "windfalls" for employers. Because of this concern the Economic Recovery Tax Act of 1981 outlawed retroactive certifications. Since fall 1981 all new certifications have had to be requested by the employer prior to the eligible individual's employment starting date. While this should reduce the incidence of subsidizing hiring decisions that were not influenced by the tax credit's availability, it raises the cost of identifying and certifying eligibles and will no doubt reduce participation in the program.

5.3 Lessons and Recommendations

While more time and more research are required before final judgments can be made about the overall effectiveness of the NJTC and TJTC, there is sufficient experience to draw some very important conclusions.

1. A tax credit for general expansion in employment for which employers can calculate and certify their own eligibility will very quickly achieve a high participation rate.

2. Employers do expand employment and cut prices in response to such a tax credit.

3. There is a danger, however, that the employment and output expansion induced by such a credit will cause an acceleration of the rate of wage increase even while price increases are slowing down.

4. A subsidy or tax credit for employing stigmatized target groups will not attract the participation of many employers. A few employers will become very heavy users, however.

5. The necessity for government certification of a worker's eligibility (rather than employer certification with audit) is a substantial barrier to employer and employee participation in an employment subsidy program.

While improved administration should increase TJTC's impact, the program is not ever likely to become big enough to have a major effect on the overall problem of stagflation.

Wage subsidies can be major instruments for dealing with stagflation only if--

1. all or almost all employers are eligible (Perloff 1981),

2. employers are able to certify their own eligiblity,

3. a tight labor market for the target group either has a negligible impact on wage inflation (Baily and Tobin 1978) or produces a larger than average increase in labor supply (Bishop 1979),

4. the target group is large enough to encompass all or almost all job seekers needing assistance (Johnson 1981),

5. the target group is defined by nonstigmatizing criteria that are visible to employers.

Such a subsidy will maximize its cost-effectiveness, if it is marginal--e.g., pays for increases in employment above a threshold. If a wage subsidy is to be marginal, however, care must be exercised in selecting the target

group, and in defining the subsidized activity and the threshold at which the subsidy begins (Bishop and Haveman 1979, Bishop and Wilson 1981).

Our experience with the NJTC suggests that a marginal wage subsidy with these qualities can succeed in stimulating employment and lowering prices. In a pure form, however, such a subsidy may promote wage inflation. This tendency can be forestalled, however, by reducing the tax credits a firm is eligible for if its wage increases exceed some wage increase standard. Such a subsidy can be very simple to administer. To calculate its subsidy the firm would need four numbers: total wage bill this year and in the base year and total hours worked this year and in the base year.

How such a scheme would work is most easily understood by examining a specific proposal. (The specfic parameters of this proposal are illustrative.) Firms and nonprofit entities would receive a tax credit against Social Security taxes of $1.00 per hour for every hour by which total hours worked (including those worked by salaried management) at the firm in 1982 exceed total hours worked in 1981. A tax credit would also be provided in 1983 for increases in total hours worked over the higher of 1982 or 1981's hours worked. In 1984 the tax credit would be for increases in total hours worked over the highest of 1983, 1982, or 1981 hours worked. The tax credit the firm would be eligible for is reduced if its average wage (calculated by dividing total compensation by total hours worked) in 1982 was more than 6 percent greater than its 1981 wage. The threshold for the wage increase "take back" might be 11 percent in 1983 and 15 percent in 1984.

A general formula for the tax credit is

$$TC = s \sum_i \Delta H_{i_t} - u \sum_i (W_{i_t} - g\bar{W}_0) H_{i_t}$$

subject to the constraint that if $TC > 0$ and $\sum_i (W_{i_t} - g\bar{W}_0) H_{i_t} > 0$

where H_{i_t} = hours worked by people in the i^{th} job during time period t

ΔH_{i_t} = growth of employment in the i^{th} job above the threshold

W_{i_t} = hourly wage rate of the i^{th} job in time period t

$\bar{W}_0$ = the firm's average wage in the base period

s = hourly tax credit

g = wage growth standard, $g > 1$

u = take back rate

An increase in the wage rate is taxed at the rate u. This discourages wage increases above the standard. An expansion of hours that leaves the composition of employment unchanged is subsidized at the rate of s dollars per hour.

Where expansions are not proportional and the firm is in the take back region, the tax benefit depends upon the wage rate of the jobs that are expanded:

$$\frac{dTC}{dH_i} = s - u(W_{i_t} - g\overline{W}_0)$$

If, for instance, s = $1 per hour, u = .1, and $g\overline{W}_0$ = $8.00 an hour, offering an additional job paying $4.00 an hour would generate a tax credit of $1.40 per hour, expanding a job paying $12.00 would generate a credit of $.60 an hour, and expanding a job paying $18.00 an hour would generate no credit.

This pattern of declining subsidies as the wage rate rises creates an incentive for the firm to focus its employment expansion in the labor markets with the greatest surplus of workers.

This type of a marginal employment subsidy has a number of attractive features:

1. Firms are encouraged to increase employment by hiring inexperienced workers and training them rather than by increasing overtime work or bidding experienced workers away from other firms by raising wages.

2. Within each firm it tends to target the employment stimulus on the least skilled workers. (This occurs because hiring extra low wage workers lowers the average wage of the firm, and this helps the firm meet the 6 percent wage increase standard.) The increase in demand at the unskilled end of the labor market should produce large reductions in the unemployment of youth and the disadvantaged.

3. Targeting on less skilled workers is accomplished without giving low wage firms a proportionately larger subsidy.

4. Firms are encouraged to slow the rate at which they increase wage rates.

5. Both marginal and average costs of production are reduced, while simultaneously, wage increases above the standard are taxed. Penalty tax incentive based income policies (TIP's) in contrast, have the disadvantage of raising marginal and average costs, and therefore, prices of firms that violate the wage standard (Seidman 1978 and Dildine and Sunley 1978).

It is a balanced anti-inflation program. The subsidy component lowers price inflation and the wage increase "take back" lowers wage inflation.

NOTES

1. The 300,000 estimate is for the second quarter of 1978. The estimate of the relevant tax expenditure is .6 times the average 1977 and 1978 tax credit claims [$6,500 = .6 · (3,250,000,000) ÷ 300,000].

2. The Perloff-Wachter study is for 1977 so the tax expenditure per job was .6 · (2,300,000,000) ÷ 700,000 = $2,000.

3. There are seven to nine million new hires every month and 55 percent of these are under age twenty-five (Cohen and Schwartz 1979). Certainly at least 10 percent of the age group is eligible so the average monthly certification rate during FY 81 of 33,000 implies that less than 10 percent of eligible new hires receive a subsidy.

CHAPTER 6

HIRING COSTS AND EMPLOYER SEARCH: THEORY AND EVIDENCE

John M. Barron, John Bishop, and William Dunkelberg

6.1 Introduction

In a seminal paper, Walter Y. Oi (1962) proposes that, analogous to a Tobin's "q" in capital investment decisions, there is an Oi's "q" in employment decisions, with Oi's "q" measuring the degree of fixity of the labor input as indicated by the discounted positive difference between an employee's value of marginal product and the wage. This discounted difference reflects the fact that employers incur certain adjustment costs in changing the number of employees. In particular, Oi cites hiring and initial training costs. As Oi states:

> These fixed employment costs constitute an investment by the firm in its labor force. As such they introduce an element of capital in the use of labor. Decisions regarding the labor input can no longer be based solely on the current relation between wages and marginal value products but must also take cognizance of the future course of these quantities. (p. 538)

Subsequent to Oi's paper, Gary S. Becker (1975) introduced an important distinction between general and specific investments in human capital. For the employer, specific investments, such as hiring expenditures or expenditures to provide specific training to a newly hired employee, offer a positive return only if that employee remains employed by that employer. Similarly, for the employee, specific investments, such as search expenditures to locate a suitable employer or expenditures to obtain specific training, offer a positive return only if that employee remains employed by that employer. Thus, the quasi-fixity of labor can more accurately be attributed not only to hiring and specific training costs incurred by the employer but also to job search and specific training costs incurred by the new employee.

Oi's analysis of the effects of specific investments on employment decisions takes, as given, the terms of the employment contract and a level of hiring costs. One extension of Oi's analysis, considered by Dale Mortensen (1978) and Masanori Hashimoto (1981), among others, is to view contractual terms such as the timing and amount of specific training to be undertaken, the sharing of training costs between employer and employee, compensation features such as wages, pensions, and severance pay, and the conditions under which either party ends, either temporarily or permanently, the employment relation, as choice variables. The optimal contractual terms that emerge may be viewed as maximizing employers' discounted profits from the employment relation subject to the employee attaining a given discounted utility.[1]

A second extension of Oi's analysis, and one that has not received much attention, is to view the cost of hiring as a choice variable. This chapter takes this view to consider why hiring costs differ across employers and positions. To start, in section 6.2 we provide new evidence on three components of employers' hiring costs. These components are interview costs as indicated by the number of hours an employer spends per applicant interviewed, employer search as measured by the number of applicants an employer interviews prior to offering an employment contract, and the likelihood that an offer of employment is accepted as measured by the number of employment offers made.

In section 6.3, we indicate the choices involved in determining the various components of hiring costs. Section 6.4 focuses on the employer's choice of the number of applicants interviewed per acceptable applicant. An employer search model is developed, taking as given the employment contract, interview costs, and the likelihood, an employment offer is accepted. In section 6.5, factors that influence the choice of the number of applicants interviewed per acceptable applicants are discussed. A unique data set is used to test the predicted effect of these factors on the actual number of applicants an employer interviews to find an acceptable applicant. In section 6.6, we discuss our empirical findings and their implications with respect to the determination of hiring costs. We also consider how other aspects of hiring costs--interview costs and the expected number of offers to be made--affect the number of applicants interviewed per acceptable applicant. Concluding remarks and possible extensions of the analysis are presented in section 6.7.

6.2 Three Components of Hiring Costs

According to a recent, extensive survey of employers, the mean number of hours an employer and current employees spend "recruiting, screening and interviewing applicant(s)" for a position is 8.9 hours.[2] However, hiring costs differ substantially across employers and across positions. To understand what causes such differences in hiring costs to exist, it is useful to identify three components of expected hiring costs.

An employer who seeks to fill a position interviews applicants. One component of hiring costs is the hours spent by the employer and staff per applicant interviewed. This expenditure represents not only actual interview time but also the time devoted to recruiting applicants and to maintaining and reviewing application forms prior to interviewing. The average expenditure per interview, I, is 2.3 hours.

A second component of hiring costs is the expected number of applicants an employer interviews prior to making an employment offer, A. The typical employer interviews close to six applicants before an employment offer is made.

A third component of hiring costs is N, the expected number of employment offers made. The evidence suggests that an offer made is usually not rejected. Only 10 percent of employers make more than one offer of employment in

order to fill a vacancy. However, for those employers who do make more than one offer, the average number of employment offers is slightly over three.

Total expected hiring costs, as measured by the number of hours, H, an employer and staff spend to fill one position are related to the three components: interview time per applicant, I; the expected number of applicants interviewed per acceptable applicant, A; and the expected number of offers to be made, N, by the following identify:

$$(1) \qquad H \equiv I \cdot A \cdot N$$

The task of the next section is to indicate what choices the employer makes with regard to each of the three components of hiring costs.

6.3 Choices that Affect Hiring Costs

Ignoring discounting, part of interview costs I is the expected cost of contacting one applicant, r/θ, where r is the employer's recruiting expenditures in hours per period, θ is the constant probability of an individual applying for employment during a very small time interval ε, and $1/\theta$ is the expected time it takes to locate an applicant. The employer's choice of a rate of recruiting expenditures and the terms of the employment contract advertised, as well as existing labor market conditions, affect the probability that an individual will apply. That is, $\theta = h(r, \eta)$, h_1, $h_2 > 0$, where η is a shift parameter reflecting terms of the employment contract and labor market conditions.

Assume that employers do not know prior to a period of employment the true value of labor services offered by a particular individual, and that those values differ across individuals.[3] If employment contracts that specify wage payments contingent on the discovered value of labor services are ruled out, then there is a gain to the employer gathering information prior to hiring about the true value of labor services offered by a particular applicant and across applicants.[4] How these information gathering activities add to interview costs I will now be examined.

Consider first the employers' gathering of information on the true value of labor services offered by a particular applicant. At a cost of s, an employer can have an application form filled out and can review it. For a particular applicant, let Q_s denote a screening index of qualifications derived from information on the application form. Let V denote the true (unknown) value of labor services offered by the individual. The information obtained by the application form is useful in predicting the true value of labor services offered in that $E(V|Q_s = q_s)$ is increasing in q_s. One choice an employer makes in this screening process is the expenditure on application form acquisition and review, s. A greater expenditure improves the accuracy of the prediction of the true value of labor service offered.

A second choice the employer makes in the screening process is the choice of a reservation screening index of qualifications, q_s^*, with only individuals

with a screening qualifications index of not less than q_s^* being offered an interview.[5] The probability that an applicant is interviewed, ϕ, depends inversely on this choice of q_s^*. Ignoring discounting, the expected cost associated with the interview of one applicant, I, can now be represented by

$$(2) \qquad I = (r/\phi\theta) + (s/\phi) + i$$

where i is the cost in hours to actually interview an applicant. The expenditure i, like s, improves the accuracy of the prediction of the true value of labor services provided.[6]

Equation (2) relates the choices of recruiting, screening, and interview costs (r, s, and i, respectively), the terms of the contract, and the reservation screening index of qualifications (q_s^*) to the expected interview costs, I. We now consider choices involved in the second component of expected hiring costs, the expected number of applicants interviewed per applicant, found to be acceptable, A.

For a particular applicant, let Q_j denote an interview index of qualifications derived from information obtained during an interview. Assuming $E(V|Q_j = q_j)$ is increasing in q_j, the employer chooses a reservation interview index of qualifications, q_j^*; only those individuals interviewed with qualifications at least as great as q_j^* are offered employment. Let α denote the probability that an applicant who is interviewed is offered employment. Since α is inversely related to q_j^*, the expected number of applicants interviewed per acceptable applicant, $A = 1/\alpha$, is directly related to the choice of q_j^*.[7] Note also that the expected number of applicants interviewed per acceptable applicant, for a given q_j^*, depends on the nature of the distribution of qualifications across applicants interviewed, which reflects back on such choices as q_s^* and r in the screening process, as well as interview costs i.

The final component of hiring costs is the expected number of employment offers made, N. The probability that an employment offer is accepted, β, and thus N, is affected by the employer's choice of the terms of employment contract as well as other factors.[8]

The expected hiring costs, H, can now be represented by (ignoring discounting)

$$(3) \qquad H = I \cdot A \cdot N = \left(\frac{r}{\phi\theta} + \frac{s}{\phi} + i\right)\left(\frac{1}{\alpha}\right)\left(\frac{1}{\beta}\right)$$

In the next section, we consider in isolation the choice of q_j^*, which affects the expected number of applicants seen per acceptable applicant, A. In subsequent sections, the theory's implications concerning the determinants of A and, thus, total hiring costs, are developed and tested.[9]

6.4 Theoretical Framework of Employer Search

Consider an employer seeking to hire for a particular position. In a given period of time ε, the probability that an applicant will be interviewed

is $\phi\theta$. The probability that an applicant interviewed will accept an employment offer is β. The probability that an individual who is interviewed is offered employment, α, is given by:

$$(4) \qquad \alpha = \int_{q_i^*}^{\infty} f(q_i)dq_i$$

where $f(v,q_i)$ is the joint probability density function of V and Q_i (conditional on the individual being interviewed) and thus $f(q_i)$ denotes the marginal probability density function of Q_i.

The time before an individual is hired, D, is a random variable having an exponential density function with parameter $\theta\phi\alpha\beta$. During this time of search for the new employer, the employer incurs recruiting, screening, and interview costs at an expected rate $r + \theta s + \phi\theta i$. In this section we take ϕ, θ, and β as given, implying that r, a, and q_s^* are not choice variables. These simplifications allow us to focus on employer search as indicated by the choice of the reservation interview index of qualification, q_i^*, and thus the number of applicants interviewed per acceptable applicant, $A = 1/\alpha$.

On hiring, the employer provides specific training of an amount T during the first period of the employee's job tenure at a cost of c(T), with $c' > 0$. During this training period, $E(V|Q_i \geq q_i^*)$ denotes the expected value of qualified labor services. Training increases the value of any individual's labor services by the common proportion Ψ, where $\Psi = g(T)$, $g(0) = 0$, and $g' > 0$. Thus after training, the employer anticipates receiving for L periods $(1+\Psi)E(V|Q_i \geq q^*)$ the expected value of a trained, qualified worker. We will assume that the employer shares neither the cost nor returns of specific training with the employee, so that the wage during training, w, also represents the wage after training.[10]

After L periods, either the new employee quits or the true value of the individual's labor services is discovered. L has a probability density function of the form

$$(5) \qquad j(L) = (\delta+\tau)e^{-(\delta+\tau)L}$$

where δ is the probability that the employee will quit and τ is the probability that the employer will discover the true supply of labor of the employee over a small length of time ε.[11] The term τ depends on the rate of monitoring costs, m, incurred by the employer and γ, a shift parameter, is that indicates the productivity of monitoring. Specifically, $\tau = k(m,\gamma)$, with $k_1, k_2 > 0$.

If the employer determines the true value of labor services before the new employee quits, then the employee is retained only if this value exceeds the wage payment. Thus the monitoring expenditures, m, reduce the expected employment length of an individual whose true value is less than w. The expected gain to the employer from hiring, G, can now be expressed as

$$G = E\Big(\int_0^D - (r+\theta s+\phi i)e^{-\rho t}\,dt + \int_D^{D+1} (E(V|Q_i \geq q_i^*) - w - c)e^{-\rho t}\,dt + \int_{D+1}^{D+1+L} ((1+\ )E(V|Q_i \geq q_i^*) - w - m)e^{-\rho t}\,dt + \frac{Pr(V \geq \frac{w}{1+\Psi}|Q_i \geq q_i^*)\tau}{\tau+\delta} \int_{D+1+L}^{D+1+L+S} ((1+\Psi)EV|V \geq \frac{w}{1+\Psi}) - w)e^{-\rho t}\,dt\Big) \qquad Q_i \geq q_i^*$$

where S, the length of employment if quitting is the only reason for ending employment, is a random variable having an exponential density function with parameter β. The optimal search strategy is characteristized by the choice of q_i^* to maximize G[12].

6.5 Factors Affecting Employer Search

According to equation (4), the choice of q_i^* implies an expected number of applicants per acceptable applicant, $A = 1/\alpha$. The choice of q_i^*, and thus A, is affected by the set of parameters $X_1,\ldots,X_n$. Specifically, let

$$(7) \qquad A = \alpha \cdot X_1^{\beta_1} \cdot X_2^{\beta_2} \ldots X_n^{\beta_n}$$

Let us now consider what theory suggests should be included in the set of parameters, $X_1, \ldots, X_n$.

Different employers face different labor market conditions. For a given probability that an applicant contacted is interviewed, ϕ, employers with a higher probability of contacting an applicant in a given period, θ, will interview a greater number of applicants per period, FREQ.[13] Such an increase in the flow of applicants to be interviewed will result in the choice of a higher reservation interview index of qualifications, as the employer can, at a lower cost, pursue a more careful selection of an individual to hire.[14] Setting X_1 = FREQ, $\beta_1 > 0$.

Larger employers are likely to be less productive in the monitoring of new employees in order to discover the true value of their labor services. This may be viewed as a reduction in γ, the parameter affecting the productivity of given monitoring expenditures. For a sufficiently small discount factor, less productive monitoring will result in the employer choosing a higher q_i^*, as a more extensive search for a new employee is sought since it is likely to be a longer period of employment before the mistakes (individuals hired who cannot perform adequately) are discovered. Setting X_2 = SIZE, where SIZE measures the number of employees, $\beta_2 > 0$ and larger employers see more applicants per acceptable applicant.[15]

The amount of specific training, T, affects expenditures during the training period, c, and the subsequent proportional gain in productivity of the employer, ψ. Assume higher specific training is chosen only if the expected gains in productivity exceed the costs. Since the increase in productivity associated with specific training is proportional to the initial value, an employer investing in more specific training places a higher value on locating individuals who are initially more productive. As a consequence, other things being equal, q_i^* and thus A are directly affected by the amount of specific training. Let X_3 = TPER, the number of hours that personnel and supervisory employees spend orienting and training an individual during the first month of employment. Let X_4 = TNPER, the number of hours that employees other than personnel and supervisory employees spend away from their normal work routine to orient and train the new employee. If TPER and TNPER are direct proxies for the amount of specific training involved in the position, then we would expect $\beta_3 > 0$ and $\beta_4 > 0$.

An increase in the variance of the distribution of the true value of labor services offered by individuals applying for a particular position increases the expected gain to the employer from the search. As a consequence, the employer will choose a higher q_i^*, and thus see more applicants per acceptable applicant. Positions requiring greater education may be positions for which there exists a wider dispersion of potential productivity across a group of applicants. Let X_5 = EDHS, X_6 = EDCO, and X_7 = EDGR, where EDHS, EDCO, and EDGR equal the exponential if individuals hired for such positions are high school graduates, have some college education, or are a college graduate, respectively; otherwise, they equal 1. We would then expect $\beta_7 > \beta_6 > \beta_5 > 0$, reflecting the fact that as educational attainment of those filling the position rises, so does the variance in the actual productivity of individuals in that position and so does the gain in seeing more applicants per acceptable applicant.

Another variable that affects the choice of q* is the likelihood that the individual will quit, δ. The greater this likelihood, other things being equal, the less the gain through extensive search to locate a new employee, and thus the choice of a lower q_i^* and a reduced number of applicants per acceptable applicant. Let X_8 = QUIT, a measure of the past quit rate for the employer. If QUIT serves as a direct proxy for the probability that a new employee will quit, then we expect $\beta_8 < 0$.

Regression (1) in table 6.1 presents an estimation of the logarithm of equation (7) utilizing the variables discussed previously. Regression (2) in table 8.1 includes additional variables that, like the education variables, could potentially indicate differences in the distribution of productivity and qualifications from which the employer is sampling. These variables indicate the employer's industrial classification and the occupational classification of the position.

6.6 Empirical Evidence on Employer Search and Hiring Costs

Before discussing the results from estimating equation (7), it is important to point out that the actual description by employers of their

TABLE 6.1

ESTIMATES OF DETERMINANTS OF NUMBER OF APPLICANTS PER ACCEPTABLE APPLICANT (A) AND HIRING COSTS (H)

Explanatory Variable[a]	Mean[b]	Dependent Variable LN(A) 1	LN(A) 2	LN(A) 3	LN(H) 4
Logarithm of the number of applicants interviewed per day LN (FREQ)	1.16	.35 (26.43)	.36 (27.21)	.30 (22.44)	.12 (7.40)
Logarithm 2f the hours personnel and supervisory staff spent orienting and training during first month LN(TPER)	19.88	.069 (5.06)	.063 (5.06)	.081 (6.83)	.14 (8.67)
Logarithm of the hours other employees spent orienting and training during first month LN(TNPER)	14.12	.036 (3.14)	.025 (2.16)	.033 (3.01)	.05 (3.61)
Logarithm of the number of employees LN(SIZE)	77.43	.042 (2.84)	.048 (3.14)	.045 (3.04)	.05 (2.60)
Logarithm of the quit rate of employer over prior quarter LN(QUIT)	.11	.004 (.27)	.007 (.50)	.01 (.07)	-.02 (.82)
New employee high school graduate EDHS	.54	.27 (6.07)	.22 (4.81)	.20 (4.65)	.15 (2.50)
New employee completed some college LDSC	.19	.47 (8.49)	.35 (6.00)	.31 (5.61)	.25 (3.47)
New employee college graduate EDCG	.05	.49 (5.66)	.35 (3.93)	.32 (3.81)	.24 (2.12)
Occupation: Professional or Technical	.05		.26 (2.93)	.28 (3.31)	.40 (3.52)
Occupation: Clerical	.37		.26 (5.02)	.27 (5.62)	.29 (4.48)
Occupation: Sales	.11		.13 (1.97)	.16 (2.52)	.21 (2.52)
Occupation: Service Worker	.15		-.03 (.42)	-.02 (.28)	-.02 (.28)
Industry: Mining and Agriculture	.02		-.08 (.60)	-.12 (.91)	-.08 (.46)
Industry: Construction	.05		-.05 (.58)	-.09 (.96)	-.13 (1.14)
Industry: Transportation and Public Utilities	.04		.17 (1.57)	.19 (1.80)	.26 (1.87)
Industry: Wholesale Trade	.09		.10 (1.23)	.12 (1.56)	.11 (1.03)
Industry: Retail Trade	.34		-.002 (.03)	.06 (.09)	-.07 (.86)
Industry: Finance, Insurance and Real Estate	.08		.09 (1.02)	.12 (1.34)	.12 (1.05)
Industry: Services	.26		.04 (.58)	.09 (1.37)	.15 (1.67)
Logarithm of the number of hours spent per individual interviewed LN(I)	2.34			-.23 (11.46)	
Employer made more than an offer MANYOFF	.10			-.43 (7.81)	.51 (6.98)
Logarithm of the starting hourly wage LN(STWAGE)				.13 (2.73)	.27 (4.18)
Constant		.94	.88	.65	.22
R^2 (adjusted)		.30	.32	.38	.17
Sample Size		2002	2002	2002	2002

NOTE: The sample for regressions 1-4 are employers who, when sampled, had hired a new employee in the previous two yers and who could provide answers to the questions in the survey concerning the hiring process. For regressions 1-3, the dependent variable is the logarithm of the ratio of the applicants interviewed for the position to the number of offers made. This is a measure of the average number of applicants per acceptable applicant. Across all employers, the mean of this variable was 5.78 applicants. For regression 4, the dependent variable is the total hours spent recruiting, screening, and interviewing applicants for the position. The absolute value of the t-statistics appears in parentheses.

[a]Unless otherwise indicated these variables are dichotomous, assuming the value 1 if the indicated requitement is met and 0 otherwise. Excluded groups are: three digit occupation codes greater than 399; four digit industry codes between 1800 and 3999.

[b]Mean of continuous variables are prior to taking the logarithm.

hiring activity was not always consistent with the theory of employer search presented. Of the employers sampled who had recently hired an individual, 28 percent stated that they did not recruit for the position.[16] For such employers, one can identify neither a positive duration of a vacancy nor hiring costs associated with filling a particular position. Similarly, identification of hiring costs and the duration of vacancy was not possible for the 3 percent of employers who stated that they were always looking. Thus, our analysis is restricted to those employers, 69 percent of all hiring employers in our sample, who could identify both the duration of vacancy and the associated hiring costs. These tended to be the larger employers in the sample.

Regression (1) and (2) in table 6.1 indicate that a 10 percent increase in the flow of applicants to an employer increases the number of applicants interviewed per acceptable applicant by between 3 and 4 percent.[17] This less than proportional effect on A implies a shorter duration of a vacancy for employers having a greater flow of applicants. With constant interview costs being I, and number of offers being N, these results would imply a 3 to 4 percent increase in total hiring costs, H. Interestingly, regression (4) indicates that total hours spent rises just over 1 percent for a 10 percent increase in the flow of applicants. This is due to an offsetting reduction in the number of hours spent per interview as the flow of applicants rises.[18]

The amount of specific training provided by an employer in the first month of employment affects employer search behavior. Increases in both hours spent by personnel and supervisory staff and hours spent by other employees orienting and training the new employee during the first month increase the number of applicants interviewed prior to an offer being made. As a result, total hours spent recruiting are greater when a larger amount of specific training is to take place [see regression (4)]. This effect on total hours is accentuated by the fact that more hours are devoted per interview for positions requiring larger amounts of orientation and training in the first month by personnel and supervisory staff.[19]

Smaller employers, who presumably can more easily monitor new employees, search less extensively to locate a new employee. This fact may help explain why greater starting wages are paid by larger employers, since more extensive searches will yield employees with higher expected productivity.[20] Note that this more careful choosing of an acceptable applicant by larger employers translates into a larger total number of hours spent recruiting for a position [see regression (4)].

Employers seeking employees with higher levels of education or filling a professional, clerical, or sales position interview more applicants per acceptable applicant, and thus devote a larger total number of hours to recruiting. On the other hand, neither the past quit rate nor the industrial classification of the firm effects the number of applicants interviewed per acceptable applicant.

There are several additional variables that are correlated with the number of applicants per acceptable applicant, but which also have substantial elements of choice inherent in them. We will now consider three of these variables that are included in regression (3) in table 6.1.

An increase in the expected cost of interviewing an applicant I, will, other things being equal, lead to a reduction in q* and thus to fewer applicants being interviewed per acceptable applicant. However, according to equation (2), a higher I may also indicate additional screening of applicants (say, a lower probability that an applicant contacted is interviewed, ϕ), so that the distribution the employer samples from when interviewing is altered. This will reduce the number of applicants interviewed per acceptable applicant, as well as imply a greater frequency of contacts with applicants if the frequency of applicants interviewed per week is held constant. Or, a higher I may represent a more extensive purchase of information during the screening or interview processes (an increase in s or i) that would also alter the nature of the distribution from which the employer samples.

A reduction in the probability that an offer is accepted will result in a reduction in q* and, thus, in A. Let MANYOFF, a dummy reflecting the case when the employer made more than one offer of employment, be an inverse proxy for . Then, we would expect MANYOFF to be inversely related to A. However, can also be considered a choice variable in the search process, so that again there is ambiguity as to what, in fact, is being captured by the correlation between MANYOFF and A.

Finally, the level of the starting wage, STWAGE, affects the search process. This may be due to its influence on the probability of an offer being accepted, , its effects on the distribution the employer is sampling from, or its effect on the information gathered at each stage of the search process.

6.7 Concluding Remarks and Extensions

Taking the view that the cost of hiring is a choice variable, this chapter investigated the role of employer search as an explanation for differences in hiring costs across employers. The evidence presented indicates that the predictions of the theory concerning the number of applicants interviewed per acceptable applicant are largely supported. Furthermore, the theory identifies important determinants of total hiring costs. For example, positions that involve a greater amount of specific training, particularly that training provided by personnel and supervisory staff, are ones for which the employer is more careful about who is hired, as indicated by a higher number of applicants interviewed per acceptable applicant, and this translates into greater total hiring costs. Similarly, employer search is more extensive and hiring costs greater if there is a larger flow of applicants per period or if the level of education sought is greater. On the other hand, smaller employers search less extensively to locate a new employee, and consequently have lower hiring costs.

Regression (3) in table 6.1 provides some interesting suggestions as to future research. A significant inverse relationship between the number of hours spent per individual interviewed and the number of applicants interviewed per acceptable applicant was found.[21] This might indicate that employers, in filling certain positions, substitute between intensive screening of applicants (much time devoted to gathering information on a

particular applicant) and extensive screening of applicants (number of applicants interviewed). But, at this stage such an interpretation is conjecture and awaits future study of hiring behavior in which variables other than the number of applicants per acceptable applicant are viewed as choice variables. This statement applies equally well to the observation that employers who make more than one offer also search a shorter period of time per offer made, or that employers offering a higher starting wage search more extensively.[22]

NOTES

1. There are other approaches to explaining long-term "implicit" employment contracts not based on specific investment. For instance, Costas Azariadis (1973) and Martin Baily (1978) suggest gains to long-term employment relations that are the result of differences in preferences for the absence of risk between employer and employee.

2. This study of the firm's search for and selection of employees is based on data from a United States Department of Labor-funded survey of 4,494 employers. In the bulk of the sample--companies with fewer than fifty employees--the respondent was the owner/manager of the establishment. In large organizations the primary respondent was the person in charge of hiring, generally the personnel officer. When the primary respondent was unable to answer a question, it was asked if someone else in the organization would have the information and that part of the interview was completed with this other official. The other respondents who were thus involved in this process were: controllers, wage and salary administrators, and line supervisors (for questions about a particular recent hire). Most interviews were obtained in the second quarter of 1980.

The survey was originally designed as the first wave of a longitudinal evaluation of the impact of the Employment Opportunity Pilot Projects (EOPP) in ten labor markets. The sites for the survey were ten EOPP pilot sites and eighteen comparison sites selected for their similarity to the pilot site. The result was a clustered sample of geographic locations that, while dispersed across the nation, over represents Gulf Coast cities and under represents the Northeast. The probability that particular firms in the selected sites would be sampled depended on size and location and ranged between .006 for some of the smallest establishments and 1.0 for most establishments with two hundred or more employees.

The needs of the EOPP evaluation and constraints on the length of the interview limited the number of questions that could be asked about the hiring and selection of a particular worker. The interview does contain a number of very useful questions on the hiring process and is, therefore, one of the best data sets available for addressing the issues raised in this paper.

3. The supplier of labor may also be uncertain as to what, in fact, has been exchanged. One response is for a supplier of labor services to sell services for a short period of time in order to discover the "fringe benefits" associated with a particular exchange, such as the work environment (see Johnson 1978).

4. Examples of contingent wage payments are piece-rate and commission wage payments or the existence of probationary periods such as the practice in academic settings of providing tenure only after an individual has been employed and his or her productivity observed for a period of time. Contingent compensation schemes require that each party know the realized value to the other party of the exchange. But, as Hashimoto and Yu (1980); Hahimoto

(1981); and Williamson, Wachter, and Harris (1975) have stressed, accurate information on the gains received by the other party is often costly to obtain. In part, this is due to the incentive each party has to misrepresent the gain, i.e., "opportunistic" behavior (see Williamson, Wachter, and Harris, 1975). As a consequence, we do not focus on contingent schemes as part of the compensation structure. Further, we assume that the wage is fixed during the hiring process. This second assumption rules out the employer offering each applicant an employment contract with appropriate differences in compensation to reflect differences in expected value of labor services provided. There is direct evidence that employment contracts are not changed during the hiring process. In a recent survey of smaller firms, it was found that few employers altered the wage offered during hiring (see Barron and Dunkelberg, 1982). Eliminating contingent wage schemes or different wages offered to different individuals during the hiring process is consistent with the fact that few employers make an offer to every applicant.

5. This sequence of screening applicants prior to interviewing implies that the cost to an employer of an interview is high relative to the cost of screening an applicant using an application form.

6. We assume it is prohibitively costly for an employer to discover the true value of labor services provided prior to a period of employment.

7. Note that we have assumed implicitly a known, time invariant distribution, $g(v,q_s,q_j)$, such that the reservation screening and interview qualification indices are not changed over time. Recent evidence consistent with this view is that very few employers do alter minimum qualifications for employment during the hiring process (see Barron and Dunkelberg, 1982).

8. Even employers offering identical contracts and having identical reservation qualifications may have different probabilities of individuals accepting an offer. This may arise if, during the interview, information is obtained on the likelihood of an individual accepting an offer. In the case of two individuals identical in productivity but differing in likelihoods of accepting employment, one employer might make an offer only to the individual with the greater likelihood of accepting employment, while a second employer might make both an offer. The second employer, who is more willing to risk rejection of an offer, is likely to have lower costs of making an offer that is turned down. These costs reflect, in part, the constraint on the making of other employment offers when an employment offer is outstanding.

9. If I and N have identical distributions across employers, then in estimating the logarithm of equation (1) using actual data on total hiring costs and number of applicants interviewed per acceptable applicant, one obtains

$$\ln H = .614 + .634 \ln A \qquad N = 2002$$
$$\qquad\quad (29.68) \qquad\quad R^2 = .30$$

This equation indicates that a substantial amount of the variation in actual hiring costs can be attributed to differences in the number of applicants interviewed per acceptable applicant.

10. This independence has been suggested by Mortensen (1978). In fact, we do find that the starting wage is not related to our measures of specific training. See footnote 17 for further details.

11. Note that if the waiting time until a new employee quits has an exponential distribution with parameter δ, and the waiting time until the true value of labor services is known has an exponential distribution with parameter τ, then the waiting time until either is exponentially distributed with parameter $(\delta+\tau)$.

12. Like r, s, and q_s^*, monitoring costs, m, training expenditures, T, and the wage, w, are not choice variables.

13. FREQ is measured by dividing the total number of applicants interviewed for the position by the duration of the vacancy. This gives the average number of applicants interviewed per day.

14. In the search model, the increase in the flow of applicants is represented by an increase in the shift parameter, η.

15. The number of employees includes both full- and part-time employees employed by the establishment during the week of December 12, 1979.

16. Of the 4,494 employers sampled, only 69 percent had recently hired a new employee, and thus answered questions concerning their most recent hire. The survey defined a hire as recent if it had occurred within the last two years.

17. Regression results were obtained when the dependent variable is the logarithm of FREQ and the independent variables are those in regression (2) in table 6.1, with the deletion, naturally, of the logarithm of FREQ, and the addition of the logarithm of the starting wage and whether more than one offer was made (MANYOFF) as independent variables. The results indicate that only employer size, MANYOFF, and several occupation and industry dummy variables are significantly correlated with the number of applicants interviewed per acceptable applicant. A 10 percent increase in employer size increases the flow of applicants interviewed per position by 1.2 percent.

It should be noted that the use of the total number of applicants in the creation of the dependent variable and one of the independent variables (applicants per day, FREQ) presents some conceptual empirical problems. First, if the number of applicants is reported with error, the coefficient on FREQ may be affected, and the explanatory power of the specifications will benefit from the correlation of the measurement errors on both sides of the equation. Secondly, FREQ will be correlated with the error term, producing biased and inconsistent estimators. The use of an instrument for FREQ is appropriate if consistent estimators are desired and the benefits of measurement error correlation eliminated. A reasonably appropriate instrument was available--the number of people contacting (by phone or in person) the firm in the three months following the recall period for the last employee hired. Using this measure in place of FREQ had no noticeable impact on the other regression coefficients and produced a somewhat lower, but significant, elasticity estimate.

18. This can be seen from a regression of the logarithm of the number of recruiting hours spent per applicant interviewed (I) on the independent variables shown in regression (2) in table 6.1, with the addition of the logarithm of the starting wage and whether more than one offer was made (MANYOFF) as independent variables. Besides the significant negative effect of the increased flow of applicants on recruiting hours spent per applicant interview, I was positively correlated with the starting wage, and with the level of specific training provided by personnel and supervisory staff (TPER).

19. See footnote 18.

20. This is shown by a regression of the logarithm of the starting wage for the position on the independent variables shown in regression (2) in table 6.1, with the addition of the logarithm of the recruiting hours spent per applicant interviewed and whether more than one offer was made (MANYOFF) as independent variables. Of interest is the fact that our measures of specific training, TPER and TNPER, are not correlated with the starting wage.

21. See regression (3) in table 6.1.

22. See regression (3) in table 6.1.

CHAPTER 7

EMPIRICAL ANALYSIS OF THE RATE OF WORKER SEPARATION

Mark E. Meitzen

This chapter presents the empirical results from estimating the rates of worker quits and discharges. First, a description of the EOPP sample (including the frequency distributions of quits and discharges in the sample) will be presented in order to get a general notion of what is being described by the empirical analysis. Second, some empirical results will be presented with the primary motive of establishing the econometric properties of the estimation techniques used.

7.1 Sample Description

Before empirically analyzing quit and discharge behavior, some features of the EOPP sample will be discussed. Data used in this study come from twenty-seven different sites around the country where the EOPP Employer Survey was conducted. Table 7.1 lists these sites and the number of firms from each site. The workers contained in the sample are the last worker hired by the firms in the low-skilled category during the period from January 1, 1978 until October 1, 1979. The EOPP Survey was conducted in spring 1980, implying that the longest period a worker can be observed for is approximately twenty-eight months (January 1978 to April 1980), while the shortest period is approximately five months (October 1979 to March 1980).

7.1.1 Frequency Distribution of Separations

In order to get a general idea of the phenomenon the empirical models of this study are attempting to explain, table 7.2 presents the frequency distribution of worker quits and discharges occurring in the EOPP sample according to length of worker employment with the firm. There are at least two reasons why these distributions should only be used for purely descriptive purposes. First, the distributions do not control for factors that may be highly correlated with the worker's propensity to quit or to be discharged (e.g., age, education, or family status of the worker). Second, the EOPP data contain a large number of right-censored observations--i.e., observations where the worker's spell of employment with the firm is not complete. Thus, a worker still with the firm on the date of the EOPP interview may separate from his/her job the day after the EOPP interview and such a separation will not be registered in table 7.2. As described previously, the nature of the EOPP sampling design implies that the shortest possible censored employment spell included in the sample would be for a worker hired on October 1, 1979 in a firm interviewed by the EOPP survey on March 1, 1980--approximately five months.

TABLE 7.1

EOPP SITES

Site	Number of Observations
1. Columbus, OH	231
2. Corpus Christi, TX	184
3. Baton Rouge, LA	196
4. Mobile, AL	193
5. Pike County, KY	114
6. Weld County, CO	66
7. Marathon County, WI	78
8 Balance of state, WA	164
9. Balance of state, MO	145
10. Toledo, OH	113
11. Cincinnati, OH	135
12. San Antonio, TX	131
13. Beaumont/Port Arthur, TX	108
14. Birmingham, AL	132
15. Buchanan/Dickenson County, VA	53
16. Alamosa County, CO	34
17. Outagamie County, WI	35
18. Skagit, Whatcom County, WA	88
19. St. Francois County, MO	89
20. New Orleans, LA	111
21. Lake Charles/Lafayette, LA	92
22. Pensacola, FL	77
23 Harlan County, KY	49
24. Logan/El Paso County, CO	31
25. Winnebago County, WI	29
26. Skamania County, WA	61
27. Grundy County, MO	69

TABLE 7.2

QUITS AND DISCHARGES BY TENURE (IN DAYS)

QUITS

	Tenure												
Quits	1-30	31-60	61-90	91-120	121-150	151-180	181-210	211-240	241-270	271-300	301-330	331-360	over 360
Number	60	53	72	45	42	60	38	23	29	9	8	0	86
% of all quits (N = 525)	11.43	10.10	13.71	8.56	8.0	11.43	7.24	4.38	5.52	1.71	1.52	0	16.38
Cumulative percentage	11.43	21.53	35.24	43.81	51.81	63.25	70.48	74.86	80.38	82.09	83.61	83.61	100.00
Number of workers with tenure at least this long	2808	2743	2664	2556	2410	2295	2158	1806	1467	1165	931	790	680
Quits as % of workers with tenure at least this long	2.14	1.93	2.70	1.76	1.74	2.61	1.76	1.27	1.98	0.77	0.86	0.0	12.65

Quits = 525
Percent of Total Sample = 18.7%

DISCHARGES

	Tenure												
Discharges	1-30	31-60	61-90	91-120	121-150	151-180	181-210	211-240	241-270	271-300	301-330	331-360	over 360
Number	10	12	13	14	2	13	5	3	2	4	2	0	10
% of all discharges (N = 50)	11.11	13.33	14.44	15.56	2.22	14.44	5.56	3.33	2.22	4.44	2.22	0	11.11
Cumulative percentage	24.44	38.88	54.44	56.66	71.10	76.66	79.99	82.21	86.65	88.87	88.87	100.00	
Number of workers with tenure at least this long	2808	2743	2664	2556	2410	2295	2158	1806	1467	1165	931	790	680
Quits as % of workers with tenure at least least this long	0.36	0.44	0.49	0.55	0.08	0.57	0.23	0.17	0.14	0.34	0.21	0.0	1.47

Discharges = 90
Percent of Total Sample = 3.2%

115

From table 7.2, it can be seen that the mode of the quit distribution occurs in the third month of employment (61-90 days) with two other noticeable peaks occurring in the first and sixth months of employment. Generally speaking, the quit distribution is relatively flat for the first six months of employment with a noticeable drop in the frequency of quits after the sixth month of employment. In fact, over 60 percent of the quits in the sample occurred in the first six months of employment.

A similar pattern emerges when the frequency distribution of discharge is examined. The mode of the discharge distribution occurs in the fourth month of employment (91-120 days), and in general, the distribution is relatively flat for the first six months of employment with a noticeable drop in the frequency of discharges thereafter. Over 70 percent of the discharges in the sample occurred in the first six months of employment.

These frequency distributions suggest that the probability of worker separations--both quits and discharges--declines with tenure on the job--i.e., quits and discharges exhibit negative duration dependence. This may be indicative of job shopping behavior on the part of firms and workers as they sample employment matches until a suitable employment match is found. If this is, indeed, the case, the first period of the employment relationship is a learning process in which the firm and worker discover if the current match is suitable, and thus, the probability of separation is greater early in the match. However, this apparent finding of duration dependence must be tempered by the fact that sample censoring is present here and the nature of the sample design implies that more separations would be observed after the fifth month of employment if all completed spells of employment were observed.

7.1.2 Variables Used in This Study

Table 7.3 provides definitions of the variables used in various models, and table 7.4 provides the means and standard deviations of these variables. In general, the vector of independent variables, $\underline{X}$, can be partitioned into three segments: worker characteristics, $\underline{W}$; firm characteristics, $\underline{F}$; and local labor market characteristics, $\underline{M}$. The first column of table 7.4 presents the variable means and standard deviations for the "full" sample--i.e., both incomplete and complete spells of employment are included--while the second column of table 7.4 presents the variable means and standard deviations for the "partial" sample of complete employment spells--i.e., a separation has occurred. Note that this is a separation of any type--quit, discharge, induced quit, and a residual "other" category.

7.2 Econometric Properties of Estimates

In this section, empirical results will be analyzed mainly from an econometric perspective in order to establish the properties of the estimation techniques used for the presentation on the quit rate equation ($r_q(t|\underline{X})$. First, an attempt will be made to justify the use of maximum likelihood estimation techniques with the main argument being that ML is best-equipped to

TABLE 7.3

DEFINITION OF VARIABLES USED IN STUDY

Variables	Definition
Firm Variables:	
INDUSTRY DUMMIES	Mining, Durable, Nondurable, Finance and Service, Transporation, and Communication (Wholesale and Retail is the excluded industry)
LOGEMPLOY	The natural logarithm of December 1, 1979 firm employment
ORIENT	The amount of time, in hours, spent by employees orienting and training the new worker during his/her first month on the job
PCTCRAFT	The percent of the firm's workers who are classified as craft workers (where percent = x/y)
PROGRESS	A dummy variable indicating whether or not there is a wage progression for the job slot the new worker was hired in no = 0, yes = 1
STARTWAGE or WAGE	The hourly wage the worker was hired at
TOPWAGE	The top wage of the job slot the worker was hired in
SCREEN	The number of hours spent by the firm recruiting, screening, and interviewing applicants for the job slot
UNION	Percent of the firm's nonsupervisory workers who are covered by a collective bargaining agreement (where percent = (x/y) * 100)
Worker Variables:	
AGE	Age of the worker at the time of hire
PROD2WK	On a scale of 1 to 100, the worker's productivity during his/her second week on the job compared to the productivity of an average experienced worker in the same job. The productivity of an average experienced worker is set at 50.
SCHOOL	How much school the worker had completed at the time of hire: 1 = no formal school 2 = grade school (1-8) 3 = some high school 4 = high school graduate 5 = some college 6 = college graduate
SEX	Dummy variable indicating sex of the worker (0 = female, 1 = male)
Market Variables:	
MKTWAGE	Average wage in manufacturing for the local labor market, 1977-1979
DMKTEMPL	Percent change in local labor market nonagricultural employment, 1977-1979 (denominator is 1977 employment)

NOTE: Data for all firm and worker variables come from the EOPP Employer Survey, while data for market variables come from Employment and Earnings, May 1980 (U.S. Bureau of Labor Statistics, 1979).

TABLE 7.4

MEANS AND STANDARD DEVIATIONS OF VARIABLES USED IN STUDY

	Full Sample	Partial Sample
LOGEMPLOY	3.027 (1.531)	2.731 (1.482)
UNION	10.36 (27.81)	8.387 (24.71)
PROGRESS	.8825 (.3221)	.8501 (.356)
LONGTOPW	90.46 (103.3)	82.96 (111.5)
PCTCRAFT	.1305 (.2134)	.1103 (.2057)
MINING	.0299 (.170)	.0255 (.158)
DURABLE	.0730 (.260)	.0929 (.291)
NONDURABLE	.0552 (.228)	.0459 (.209)
FINSERV	.338 (.471)	.316 (.465)
TRANSCOM	.0424 (.776)	.0420 (.201)
SCREEN	6.343 (29.12)	5.709 (36.13)
ORIENT	32.33 (35.90)	29.38
STARTWAGE	4.009 (1.619)	3.937 (1.618)
TOPWAGE	5.197 (1.933)	5.020 (1.865)
SEX	.455 (.498)	.483 (.499)
SCHOOL	4.166 (.965)	4.104 (.966)
PROD2WK	55.94 (22.47)	52.28 (23.13)
MKTWAGE	6.678 (.776)	6.662 (.758)
DMKTEMPL	.0895 (.0407)	.0893 (.0402)
TENURE (in days)	278.1 (170.1)	176.2 (136.9)
N	2808	785

deal with the censoring problem in the EOPP data. Second, once the case for using maximum likelihood has been made, various functional forms of $r_q(t|\underline{X})$ will be presented with the main issue being whether or not structural duration dependence can be identified. Because the emphasis of this section is on discovering the econometric properties of the estimates, little attention will be given to a meaningful interpretation of the results until section 3.

7.2.1 The Use of Maximum Likelihood

There are two primary reasons for using maximum likelihood techniques to estimate the quit rate and discharge rate equations. First, maximum likelihood produces consistent, asymptotically efficient parameter estimates that are asymptotically distributed if normally provided regularity conditions on the density function, $f(t|\underline{X})$, are met. Second, and most importantly, maximum likelihood is able to accommodate sample censoring while the use of standard regression techniques is likely to result in a sample selection bias problem. This subsection will attempt to illustrate this second proposition by comparing the results of comparable maximum likelihood (ML) and least squares (LS) estimations.

The general form of the (conditional) instantaneous rate of worker quits is:

$$(1)\quad r_q(t|\underline{X}) = m_q(t|\underline{X},s) \cdot h(t|\underline{X})$$

where

$r_q(t|\underline{X})$ = conditional instantaneous rate of quitting

$m_q(t|\underline{X},s)$ = probability that separation was a quit conditional on and the fact that a separation, s, occurred

$h(t|\underline{X})$ = the hazard of separation--i.e., the instantaneous probability of any separation occuring conditional on $\underline{X}$

To estimate $r_q(t|\underline{X})$ by maximum likelihood, a distribution must be assumed for the density of tenure on the job, $f(t|\underline{X})$. It is shown in chapter 3 of Meitzen (1982) that assuming a functional form for $r_q(t|\underline{X})$ will imply a density function, $f(t|\underline{X})$. To facilitate a comparison of ML and LS techniques, assume that the quit rate is a time-independent exponential function and that quits are the only cause of separation (i.e., $m_q(t|\underline{X},s) = 1$):

$$(2)\quad r_q(t|\underline{X}) = \exp\{\underline{X}'\underline{\beta}\}$$

$$(3)\quad r_q(t|\underline{X}) = h(t|\underline{X})$$

This implies that tenure on the job is exponentially distributed with parameter $r_q(t|\underline{X})$:

$$(4)\quad f(t|\underline{X}) = r_q(t|\underline{X}) \quad \exp\{-t\ r_q(t|\underline{X})\}$$

Under the exponential distribution, the expected length of tenure is:

$$(5)\quad E(T|\underline{X}) = \frac{1}{r_q(t|\underline{X})} = \exp\{-X'\underline{\beta}\}$$

Given the time-independent exponential assumption, the parameters, $\underline{\beta}$, can be estimated by maximum likelihood. If tenure on the job, T, is exponentially distributed with parameter $\alpha = \exp\{X\beta\}$, a least squares regression of the following type will produce estimates of $\underline{\beta}$:

$$(6)\quad T = \exp\{-\underline{X}\ \underline{\beta}\ v\}$$

where v has an exponential distribution with unit scale parameter and is distributed independent of X. Taking natural logarithms of (6) yields:

$$(7)\quad \ln T = -\underline{X}\ \underline{\beta} + \ln v$$

and the expectation, conditional on $\underline{X}$, is:

$$(8)\quad E(T|\underline{X}) = -\underline{X}\ \underline{\beta} + E(\ln v)$$

and, therefore, a least squares regression of the logarithm of tenure on $\underline{X}$ will yield estimates of $-\beta$. Thus, given a sample with all complete spells of employment (i.e., no sample censoring is present), the ML and LS techniques should result in coefficients that are the same magnitude but of opposite signs. The only difference between the two should be the value of the constant term if $E(\ln v) \neq 0$.

Assume for the moment that the full sample contains only complete employment spells which end in quits. The first two columns of table 7.5 can be thought of as ML and LS estimates under this assumption as the 525 quits in the EOPP sample were used to obtain the coefficient estimates in columns 1 and 2. Inspection of the first two columns reveals that in all cases except one, the ML and LS coefficients have opposite signs. In the case where this is not true (DURABLE), both ML and LS coefficients are highly insignificant. Furthermore, for the coefficients that are statistically significant, the absolute values of ML and LS coefficients are very similar, and therefore, it can be concluded that the use of either ML or LS is acceptable. However, as in the EOPP data, once incomplete spells of employment are present in the sample, this conclusion is no longer valid. In chapter 3, section 4 of Meitzen, it is shown that when incomplete spells of employment are present, the use of least squares imposes a sample selection rule that results in biased coefficient estimates while maximum likelihood can cope with censored observations, and thus, produce unbiased coefficient estimates by having incomplete spells of employment contribute a "probability of no separation" term to the likelihood function. To illustrate this point, incomplete spells of employment will be allowed in the sample and two common sample selection rules will be imposed so that LS can deal with the incomplete spells:

Rule 1: Allow only completed spells of employment--i.e., only those spells that have resulted in a quit are allowed.

TABLE 7.5

COMPARISON OF MAXIMUM LIKELIHOOD AND LEAST SQUARES ESTIMATES

	Partial Sample				Full Sample			
	$MLr_q(t\|X)$		RULE 1 LS		$MLr_q(t\|X)$		RULE 2 LS	
CONSTANT	-4.460	(-8.487)	3.896	(7.760)	-5.221	(-9.844)	4.713	(26.615)
LOGEMPLOY	.1226	(3.645)	- .1205	(-3.648)	- .1373	(-4.174)	- .0169	(-1.595)
UNION	- .000596	(- .260)	.00120	(.527)	- .00518	(-2.304)	.000758	(1.259)
PROGRESS	- .0519	(- .381)	.158	(1.228)	- .5326	(-3.958)	.2296	(4.700)
LONGTOPW	- .00106	(-1.963)	.00134	(2.870)	- .000761	(-1.926)	.000357	(2.375)
PCTCRAFT	- .239	(- .929)	.247	(1.041)	- .836	(-3.438)	.2344	(3.324)
MINING	- .0424	(- .122)	.158	(1.445)	- .0135	(- .045)	- .00646	(- .0674)
DURABLE	- .0512	(- .243)	- .124	(- .615)	.295	(1.406)	- .188	(-2.911)
NONDURABLE	- .214	(- .873)	.0258	(.107)	- .0871	(- .356)	- .0301	(- .434)
FINSERV	- .0415	(- .405)	.0935	(.934)	- .0612	(- .597)	.0569	(1.654)
TRANSCOM	.183	(.809)	- .220	(- .991)	.298	(1.299)	- .0321	(- .421)
SCREEN	- .0162	(-2.099)	.0214	(2.878)	- .0157	(-2.463)	- .00210	(-4.169)
ORIENT	- .000987	(.577)	.00225	(1.567)	- .00508	(-3.499)	.00237	(5.603)
WAGE	- .0164	(- .412)	.00229	(.0579)	- .0758	(-1.926)	.0191	(1.668)
SEX	.159	(1.626)	- .193	(-1.986)	- .0214	(- .211)	- .100	(-3.093)
AGE	.000881	(.164)	- .00322	(- .611)	- .0251	(-4.952)	.00475	(3.161)
SCHOOL	.109	(-2.198)	.125	(2.635)	.0319	(.664)	.259	(1.625)
PROD2WK	- .00223	(-1.052)	.00583	(2.903)	- .00824	(-4.057)	.00376	(5.595)
MKTWAGE	- .0377	(- .616)	.0236	(.399)	.0652	(1.077)	- .00932	(-.469)
DMKTEMPL	.203	(.179)	- .0670	(- .061)	.566	(.516)	- .233	(.618)
Dependent Variable	Quit Rate		Log of Tenure		Quit Rate		Log of Tenure	
	X^2 = 50.77		R^2 = .1543		X^2 = 164.61		R^2 = .0669	
	LOG L = -3241.073				LOG L = -4277.752			
	$\bar{r}_q$ =.003976				$\bar{r}_q$ = .006723			
	N=525		N = 525		N = 2808		N = 2808	

Rule 2: Include all observations and treat incomplete spells as if a quit occurred at the end of observed tenure.

Rule 1 is imposed in column 2 of table 7.5 where the LS regression is performed on only employment spells that resulted in a quit, while Rule 2 is imposed in column 4 of table 7.5 where the LS regression is performed on the full sample of complete and incomplete spells of employment. The ML estimates when the full sample is used are found in column 3 of table 7.5.

A comparison of columns 2 and 3 reveals that when Rule 1 is imposed, the LS coefficients are generally smaller in absolute value than the ML coefficients. In only one case (SCREEN) is the LS estimate greater in absolute value than the ML estimate when the ML estimate is statistically significant. Five coefficients (LOGEMPLOY, SEX, AGE, SCHOOL, MKTWAGE) have the same sign in the LS and ML models with the LS coefficients being statistically significant in three of these cases (LOGEMPLOY, SEX, SCHOOL) and the ML coefficient being significant in two of these cases (LOGEMPLOY, AGE).

A comparison of columns 3 and 4 reveals that when Rule 2 is imposed, the LS coefficients are, again, generally smaller in absolute value than the ML coefficients. Only once is the LS coefficient larger in absolute value (SEX), and in this case, the ML coefficient is not statistically significant. Six coefficients have the same sign in the LS and ML models with the LS coefficient being statistically significant in two cases (SCREEN, SEX) and the ML coefficient being statistically significant in two cases (LOGEMPLOY, SCREEN).

Before drawing any conclusions, it must be asked whether the ML-LS comparison of columns 3 and 4 is valid since, when LS is performed on the full sample, all types of separations are present (e g., quits, discharges, induced quits), while the ML estimates deal only with the quit rate, $r_q(t|\underline{X})$. In order to overcome this doubt, the overall hazard $h(t|\underline{X})$ that includes all causes of separation was estimated by ML, and the results are compared to the Rule 2 LS in table 7.6. Although the coefficients or $r_q(t|\underline{X})$ and $h(t|\underline{X})$ differ, the same conclusions apply when the coefficients of $h(t|\underline{X})$ are compared to the coefficients of the Rule 2 LS. In all but three cases (DURABLE, FINSERVE, SCHOOL), the LS are smaller in absolute value than the ML coefficients and in none of these three cases is the ML coefficient statistically significant. Five coefficients have the same sign in the LS and ML models with the LS coefficient being statistically significant in one case (SCREEN), and the ML coefficient being statistically significant in two cases (LOGEMPLOY, SCREEN).

In sum, the results of tables 7.5 and 7.6 show that when using this particular data set, imposing a sample selection rule and using least squares produces coefficient estimates that are closer to zero than the maximum likelihood procedure which adjusts for the incomplete (censored) employment spells. Thus, assuming that the ML technique does produce unbiased coefficient estimates, in this particular case, no matter which sample selection rule is imposed, the use of LS techniques results in coefficient estimates that are generally biased toward zero. Although the magnitude of the bias may depend on the assumed distribution of the density function (exponential in this case,

TABLE 7.6

COMPARISON OF OVERALL HAZARD AND RULE 2 LEAST SQUARES

	ML $h(t\|\underline{X})$		RULE 2 LS	
CONSTANT	-5.253	(-10.641)	4.713	(26.615)
LOGEMPLOY	- .128	(- 4.237)	- .0169	(- 1.595)
UNION	- .00476	(- 2.331)	.000758	(1.259)
PROGRESS	- .409	(3.180)	.229	(4.700)
LONGTOPW	- .00114	(- 1.976)	.000357	(2.375)
PCTCRAFT	- .6638	(- 3.105)	.2344	(3.324)
MINING	- .0411	(- .134)	- .00646	(- .0674)
DURABLE	.141	(.726)	- .188	(- 2.911)
NONDURABLE	- .229	(- .985)	- .0301	(- .434)
FINSERV	- .0445	(- .472)	.0569	(1.654)
TRANSCOM	.172	(.787)	- .0321	(- .421)
SCREEN	- .0138	(- 2.458)	- .00210	(- 4.169)
ORIENT	- .00403	(- 3.121)	.00237	(5.603)
WAGE	- .0635	(- 1.771)	.0191	(1.668)
SEX	.137	(1.548)	- .100	(- 3.093)
AGE	- .0185	(- 4.120)	.00475	(3.161)
SCHOOL	.00455	(.100)	.0759	(1.625)
PROD2MK	- .0111	(- 5.936)	.00376	(5.595)
MKTWAGE	.0755	(1.345)	- .00932	(- .469)
DMKTEMPL	.589	(.581)	- .233	(- .618)
Dependent Variable	Separation Rate		Log of Tenure	
	N = 2808		N = 2808	
	X^2 = 173.05		R^2 = .0569	
	Log $\underline{L}$ = -4923.665			
	h = .0007375			

t statistics in parentheses.

this downward bias is a general feature of sample selection rules since the expected value of a distribution truncated from above will be less than the expected value of an untruncated distribution. Hence, in the case of analyzing spells of employment, these downward-biased coefficients will result in the analyst concluding that variables have a smaller impact on tenure.

7.2.2 Comparisons of Functional Forms

In this subsection, the performances of various functional forms for the quit and discharge equations are compared with a focus on two issues relating to the presence of omitted variables (i.e., uncorrected sample heterogeneity) in the empirical models: (1) does the existence of omitted variables result in a significant bias of the estimates of the β coefficients and (2) can structural duration dependence be detected in either the quit or the discharge equation? The functional forms are:

Model 1: $r_1(t|\underline{X}) = \exp \underline{X}'\underline{\beta}$, a time-independent model.

Model 2: $r_2(t|\underline{X}) = v \cdot \exp \underline{X}'\underline{\beta}_2$, a time-independent model that includes a multiplicative error term, v, to capture the effects of omitted variables.

Model 3: $r_3(t|\underline{X}) = \exp \underline{X}'\underline{\beta}_3 \cdot \exp \partial t$, a Gompertz hazard model that allows duration dependence.

To analyze the effects of omitted variables on the estimates of $\underline{\beta}$, Models 1 and 2 will be compared. The time-independent specification of Model 1 does not allow for the effects of omitted variables, while Model 2 captures the effects of omitted variables through the Gamma-distributed error term, v, which has a unit mean and variance of δ^2. In the terminology of section 2.3 of chapter 3 of Meitzen, the 1981 Model 1 represents the "apparent" hazard, and Model 2 represents the "true" hazard.

Recall, Lancaster and Nickell (1980, p. 151) concluded that the effect of omitted variables on the coefficients of included variables is to bias these coefficients toward zero but that the bias is not likely to be serious. From equations (29a) and (30a) of section 2.3 of chapter 3 of Meitzen, the coefficients of the "true" hazard and the "apparent" hazard were shown to be:

$$(9)\quad \frac{\partial \ln r_2(t|\underline{X})}{\partial x_j} = \beta_{j2} \qquad \text{(True)}$$

$$(10)\quad \frac{\partial \ln r_1(t|\underline{X})}{x_j} = \beta_{j2}[1 + \delta^2 \exp \underline{X}'\underline{\beta}_2 \, t]^{-1} = \beta_{j1} \qquad \text{(Apparent)}$$

Thus, $\beta_{j1} < \beta_{j2}$ and the degree of the bias depends on δ^2 with larger values for δ^2 causing a larger downward bias in β_{j1}.

To illustrate this proposition, versions of Models 1 and 2 that include the same variables are presented in table 7.7 with columns 1 and 2 presenting the quit equation results and columns 3 and 4 presenting the discharge equation results. Focusing first on the quit equation results, notice in column 2 that δ^2 is very small and statistically insignificant, which leads to the prediction that the values of the β_1's and β_2's are very close. Inspection of the coefficients in columns 1 and 2 reveals that, in general, β_{j2} is only slightly larger in absolute value than β_{j1}. In fact, if an average of the ratio $\frac{\beta_{j1}}{\beta_{j2}}$ is taken over all coefficients in the quit equation, it is found that, on average, β_{j1} is .993 times the size of β_{j2} (i.e., $\bar{\beta}_R = \frac{1}{J} \sum_{j=1}^{J} \frac{\beta_{j1}}{\beta_{j2}} = .993$). Thus it is reasonable to conclude that there is virtually no omitted variable effect on the coefficients of the quit equation, and according to this criterion, Model 1 is an appropriate estimator for the β's.

Turning attention to the discharge equation, column 4 reveals that δ^2 is very large and statistically significant, and therefore, it is suspected that the β_1's will be noticeably closer to zero than the β_2's. Inspection of columns 3 and 4 appears to confirm this suspicion as the coefficients of Model 1 are generally closer to zero than are the coefficients of Model 2, and on average, β_{j1} is only .786 times the size of β_{j2} (i.e., $\beta_R = .786$). Thus, there may be evidence for preferring Model 2 over Model 1 in the discharge equation when the bias of the β's is the choice criterion. However, it is difficult to determine when the downward bias of the β_1's becomes serious (i. e., what value of β_R establishes a "cutoff" point below which Model 2 is preferred?), and furthermore, given that most of the coefficients of the discharge equation are statistically insignificant, it is not clear whether there should be concern over the bias of these coefficients.

Focusing now on the second issue of whether or not duration dependence is evident in either the quit or the discharge equation, we must observe that an identification problem exists since, with only one employment spell observed for each worker, time dependence of the hazard function cannot be distinguished from the effects of omitted variables on the measured hazard. To illustrate this problem, compare Models 2 and 3:

Model 2: $r_2(t|\underline{X}) = v \cdot \{\exp \underline{X}'\underline{\beta}_2\}$

Model 3: $r_3(t|\underline{X}) = \exp\{\underline{X}'\underline{\beta}_3\} \cdot \exp \delta t$

These models can be thought of as restricted versions of a proportional hazards model that allows for the effect of error:

$$(11) \quad r(t|\underline{X}) = v \cdot \Psi_1(\underline{X}) \cdot \Psi_2(t)$$

where in Model 2, $\Psi_2(t) = 1$ and in Model 3, $v = 1$. Or, similiarly, in Model 2 it is being assumed that $\delta^2 \neq 0$ and $\delta = 0$ while in Model 3 it is being assumed that $\delta^2 = 0$ and $\delta \neq 0$. Thus, Models 2 and 3 represent different functional form assumptions for capturing time variation of hazard.

TABLE 7.7

COMPARISON OF MODELS 1 AND 2 FOR QUIT AND DISCHARGE EQUATIONS

	Apparent β_{J1}		True β_{J2}		Apparent β_{J1}		True β_{J2}	
	Model 1 Quit		Model 2 Quit		Model 1 Discharge		Model 2 Discharge	
CONSTANT	-5.221	(-9.84)	-5.144	(-9.10)	-8.552	(-6.33)	-8.347	(-5.17)
LOGEMPLOY	.137	(-4.17)	- .139	(-4.12)	- .0629	(- .81)	- .0732	(- .75)
UNION	- .00518	(-2.30)	- .00524	(-2.30)	- .00268	(- .55)	- .00334	(1.17)
PROGRESS	- .533	(-3.96)	- .541	(-3.90)	.540	(1.24)	.592	(1.17)
LONGTOPW	- .000761	(-1.93)	- .000772	(-1.30)	- .00404	(-2.04)	- .00459	(-2.03)
PCTCRAFT	- .836	(-3.44)	- .855	(-3.42)	- .101	(- .22)	.0215	(.03)
MINING	- .0135	(- .05)	- .0222	(- .06)	- .195	(- .30)	- .434	(- .55)
DURABLE	.295	(1.41)	.296	(1.39)	- .609	(-1.19)	- .698	(-1.16)
NONDURABLE	- .0871	(- .36)	-.0941	(- .38)	-1.140	(-1.53)	-1.280	(-1.54)
FINSERV	- .0612	(- .60)	-.0581	(- .55)	.0530	(.22)	.138	(.45)
TRANSCOM	.298	(1.30)	.299	(1.28)	- .577	(- .78)	- .592	(- .72)
SCREEN	- .0157	(-2.46)	- .0161	(-2.46)	- .00356	(- .34)	- .00329	(- .29)
ORIENT	- .00508	(-3.50)	- .00517	(-3.47)	- .000428	(- .15)	.000869	(.23)
WAGE	- .0758	(-1.93)	- .0766	(-1.91)	.0254	(.29)	.00267	(.03)
SEX	- .0214	(- .22)	- .0151	(- .13)	1.028	(4.23)	1.350	(4.20)
AGE	- .0251	(-4.95)	- .0255	(-4.88)	.0895	(.93)	.0163	(1.25)
SCHOOL	.0319	(.66)	.0299	(.61)	- .166	(-1.41)	- .195	(-1.33)
PROD2WK	- .00824	(4.06)	- .00840	(-4.00)	- .0287	(-5.68)	- .0339	(-5.36)
MKTWAGE	.0652	(1.08)	.0635	(1.02)	.117	(.78)	.115	(.62)
DMKTEMPL	.566	(.52)	.596	(.53)	.465	(.18)	1.453	(.46)
			.137 σ^2	(.92)	6.733 σ^2	(3.57)		
Quits or Discharges	525		525		90		90	
N	2808		2808		2808		2808	
χ^2	164.61		164.84		74.44		79.08	
Log Likelihood	-4277.752		-4277.641		-868.941		-866.622	
$\bar{r}$	.0006723		.0006723		.0001152		.0001152	

t-statistics in parentheses.

In Model 2, time variation is captured by the "omitted variable effect," v, since the average of v over sample survivors at duration t, $\bar{v}$, changes with t. For example, suppose the true quit hazard (i.e., conditional on all explanatory variables, X*) is time-independent while the apparent quit hazard (i.e., conditional on only a subset of all explanatory variables, $\underline{X}$) leaves out a variable such as "inherent propensity to quit" which is thus captured in v. In this case, workers with a higher inherent propensity to quit will depart from their jobs early, leaving only workers with lower inherent propensities to quit in their jobs as duration increases, and therefore, the average of the inherent propensities to quit across surviving members of the sample, $\bar{v}$, decreases as duration increases. In Model 3, time variation is captured by the "structural duration dependence parameter," δ. For example, in the context of a job-matching model, δ would represent the decline in the probability of separation due to a learning process as firms and workers are likely to learn of the bad aspects of the employment match early, and therefore, separation is most likely to occur near the beginning of the match. Hence, the problem illustrated here is one of identification: given that duration dependence is observed, is it due to leaving out a variable which measures the inherent propensity of workers to quit or is it due to the learning process associated with job matching?

Examples of this issue are found in tables 7.8 where Models 1, 2, and 3 are compared for the quit and discharge equations. Examining the portion of the table on quit equations, two comments are in order. First, the values of the log of the likelihood are virtually identical for all three models, and therefore, neither the inclusion of an error term nor the allowance for duration dependence can improve upon the overall explanatory power of the time-independent model. Second, the coefficients of Model 2 are just slightly larger than the almost identical coefficients of Models 1 and 3. These two observations lead to the conclusion that the time-independent assumption of Model 1 appears to provide a reasonable explanation for worker quits in the EOPP sample. This conclusion, however, is conditional on the very special nature of the EOPP sample: newly hired, low-skilled workers are being followed for a relatively short period of time (at most, a worker is followed for the first 2.5 years on the job). The time-independent Model 1 may not perform as well with, for example, data sets that follow the worker over a longer period of time.

The results of Models 1, 2, and 3 for the discharge equation are presented in table 7.8, the section on discharge equation 3, and the most striking finding is the large and statistically significant value of ^{2} in Model 2. Furthermore, column 3 reveals a duration dependence effect that is statistically significant at the 20 percent level. Inspection of the values of the log of the likelihood reveal almost identical values for Models 2 and 3 (which are both somewhat larger than the value for Model 1), which suggests that both of these models are slightly more powerful than the basic time-independent model. Overall, the discharge equation performs rather poorly, and therefore, it may be a mistake to attribute the falling hazard to structural duration dependence. Thus, Model 2 appears to be the preferred functional form for the discharge equation.

TABLE 7.8

COMPARISON OF MODELS 1, 2 AND 3

	By Quit Equation					
	Model 1		Model 2		Model 3	
CONSTANT	-5.221	(-9.84)	-5.144	(-9.10)	-5.217	(-9.80)
LOGEMPLOY	- .137	(-4.17)	- .139	(-4.12)	- .137	(-4.17)
UNION	- .00518	(-2.30)	- .00524	(-2.30)	- .00517	(-2.30)
PROGRESS	- .533	(-3.96)	- .541	(-3.90)	- .532	(-3.95)
LONGTOPW	- .000761	(-1.93)	- .000772	(-1.30)	- .000759	(-1.29)
PCTCRAFT	- .836	(-3.44)	- .855	(-3.42)	- .836	(-3.44)
MINING	- .0135	(- .05)	- .0222	(- .06)	- 0139	(- .05)
DURABLE	.295	(1.41)	.296	(1.39)	.295	(1.41)
NONDURABLE	- .9871	(- .36)	- .0941	(- .38)	- .0872	(- .36)
FINSERV	- .0612	(- .60)	- .0581	(- .55)	- .0607	(- .59)
TRANSCOM	.298	(1.30)	.299	(1.28)	.297	(1.30)
SCREEN	- .0157	(-2.46)	- .0161	(-2.46)	- .0157	(-2.46)
ORIENT	- .00508	(-3.50)	- .00517	(-3.47)	- .00507	(-3.49)
WAGE	- .0758	(-1.93)	- .0766	(-1.91)	- .0757	(-1.93)
SEX	- .0214	(- .22)	- .0151	(- .15)	- .0212	(- .22)
AGE	- .0251	(-4.95)	- .0255	(-4.88)	- .0251	(-4.94)
SCHOOL	.0319	(.66)	.0299	(.61)	.0319	(.67)
PROD2MK	- .00824	(-4.06)	- .00840	(-4.00)	- .00824	(-4.05)
MKTWAGE	.0652	(1.08)	.0635	(1.02)	.0651	(1.07)
DMKTEMPL	.566	(.52)	.596	(.53)	.564	(.51)
			.137 σ^2	(.92)	- .0000260$^{\alpha}$	(.09)
Quits	525		525		525	
N	2808		2808		2808	
X^2	164.61		164.84		164.62	
Log Likelihood	-4277.752		-4277.641		-4277.748	
$\bar{F}$	.0006723		.0006723		.0006723	

t-statistics in parentheses.

TABLE 7.8 (Continued)

COMPARISON OF MODELS 1, 2 AND 3

	By Discharge Equation					
	Model 1		Model 2		Model 3	
CONSTANT	-8.552	(-6.33)	-8.347	(- .17)	-8.333	(-6.18)
LOGEMPLOY	- .0629	(- .81)	- .0732	(- .75)	- .0724	(- .92)
UNION	- .00268	(- .55)	- .00334	(- .57)	- .00239	(- .49)
PROGRESS	.540	(1.24)	.592	(1.17)	.565	(1.30)
LONGTOPW	- .00404	(-2.04)	-.00459	(-2.03)	- .00387	(-1.97)
PCTCRAFT	- .101	(- .22)	.0215	(.03)	- .108	(- .24)
MINING	- .195	(- .30)	-.434	(- .55)	- .181	(- .27)
DURABLE	- .609	(-1.19)	-.698	(-1.16)	- .587	(-1.15)
NONDURABLE	-1.140	(-1.53)	-1.280	(-1.54)	- 1.132	(-1.51)
FINSERV	.0530	(.22)	.138	(.45)	.0751	(.31)
TRANSCOM	- .577	(- .80)	-.592	(- .72)	- .589	(- .80)
SCREEN	- .00356	(- .34)	-.00329	(- .29)	- .00386	(- .37)
ORIENT	- .000428	(- .15)	-.000869	(- .23)	- .0000772	(- .32)
WAGE	.0254	(.29)	.00267	(.03)	.0209	(.24)
SEX	1.028	(4.23)	1.350	(4.20)	1.033	(4.24)
AGE	.0895	(.93)	.0163	(1.25)	.00993	(1.03)
SCHOOL	- .166	(-1.41)	-.195	(-1.33)	- .162	(-1.38)
PROD2WK	- .0287	(-5.68)	-.0339	(-5.36)	- .0282	(-5.61)
MKTWAGE	.117	(.78)	.115	(.62)	.114	(.76)
DMKTEMPL	.465	(.18)	1.453	(.46)	.369	(.14)
			6.733 σ^2	(3.57)	- .00138 α	(1.80)
Discharges	90		90		90	
N	2808		2808		2808	
X^2	74.44		79.08		77.99	
Log Likelihood	-868.941		-866.622		-867.167	
$\bar{F}$	.0001152		.0001152		.0001152	

t-statistics in parentheses.

The results of Models 2 and 3 of the quit and discharge equations suggest that the omitted variables effect "predicts" the duration dependence effects in that a small δ^2 is associated with a small δ (quit equation), and a large δ^2 is associated with a large δ (discharge equation). This type of finding is also reported by Lancaster and Nickell (1980), who state that:

> In a sense the effect of time variation in the hazard and the systematic error are "collinear." This is particularly clear in Lancaster (1979) where assuming (a) no error but possible time dependence and, (b) possible error and no time dependence give almost identical log likelihoods. This suggests that there may be a problem of identification . . . . (p. 149)

Therefore, an appropriate conclusion of the empirical results presented here is that in the EOPP sample, measured duration dependence is probably due to the effects of omitted variables since in both the quit and discharge equations, δ^2 is a good predictor of the magnitude and statistical significance of the duration coefficient, δ.

7.2.3 Summary

Section 7.2.1 discussed the econometric problems posed by the incomplete or right-censored employment spells contained in the EOPP sample. When incomplete spells are present in the sample, least squares regression methods require a sample selection rule to be imposed, with two common rules being (1) use only completed employment spells and (2) include incomplete employment spells and treat them as if they were complete at the end of the observation period. In contrast to these least squares techniques, the maximum likelihood procedure used in this study allows censored observations to contribute a term to the likelihood function, which accounts for the fact that no separation has occurred in these cases. It was found that compared to this maximum likelihood method, least squares under both sample selection rules results in coefficient estimates that are closer to zero.

In section 7.2.2, two issues related to the presence of uncorrected sample heterogeneity were discussed. First, the effects of uncorrected sample heterogeneity on coefficient estimate were explored and in the discharge equation, where a significant amount of uncorrected sample heterogeneity was detected, a downward bias in the coefficient estimates was discovered. Second, it was found that the degree of uncorrected sample heterogeneity "predicts" measured duration dependence. In the quit equation, where no heterogeneity was detected, no measured duration dependence was evident, while the effects of heterogeneity in the discharge equation produced negative measured duration dependence.

Finally, as discussed in Appendix B, the EOPP sampling design may result in choosing employment spells that are longer than spells chosen randomly. The econometric methodology used in this study does not correct for this length-bias problem, and therefore, the results obtained in this study must

be conditioned upon the fact that the employment spells used to estimate the probability of separation are longer than spells chosen at random. This implies that the coefficient estimates may be upwardly biased.

The section that follows provides an interpretation of the results from a labor economics perspective. First, the results of the quit equation will be discussed, and second, the results of the discharge equation will be discussed.

When viewing these results, it is important to keep in mind the special nature of the EOPP sample. Specifically, it should be remembered that the workers in the sample are in low-skilled occupations, and in order to be in the sample, the worker must have been hired within a specified time period (January 1, 1978 to October 1, 1979). Furthermore, given the relatively short time period in which these workers were observed (anywhere from five to twenty-eight months), it can be said that this study is analyzing the separation rates of recently hired low-skilled workers.

7.3 Interpretation of the Quit Results

This subsection presents the results from estimating the quit rate equation, $r_q(t|\underline{X})$. The discussion focuses on interpreting the coefficient estimates within a hypotheses testing framework. Where applicable, the results will be compared with the results of other studies. The issue of duration dependence of the hazard function will also be raised again with an emphasis on drawing implications from the results and comparing the results with some other findings.

7.3.1 Quit Equation Hypotheses

First, hypotheses regarding the signs of coefficients and the resulting estimates are presented. It is important to keep in mind that all of these statements are made conditional on all other influential factors being held constant. However, in some cases, the set of explanatory variables may not be able to control adequately for factors that have a systematic effect on the quit rate, and thus, coefficient estimates are likely to suffer from omitted variables bias. It will be pointed out when such cases are apparent. In general, the vector of explanatory variables, $\underline{X}$, can be partitioned into four categories of variables:

1. General: General firm characteristics that are directly related to the last worker hired
2. Specific: Firm characteristics that are specifically related to the last worker hired
3. Worker: Characteristics of the last worker hired
4. Market: Local labor market characteristics

The section that follows contains the hypothesized signs of the coefficient for each variable and a brief explanation of the meaning behind each hypothesis. A more detailed explanation is given when the results are presented.

General Variables

- LOGEMPLOY < 0. Firm size can be thought of as a proxy for internal job ladder development. Larger firms are more likely to offer training and advancement opportunities to workers than small firms, and hence, the employment relationship tends to be relatively permanent.

- UNION < 0. The percent of unionized workers represents the union's strength in the firm. Aside from an ability to capture economic rents in the form of higher wages, unionization is positively related to the quality of working conditions as, to paraphrase Freeman (1980), the union grievance machinery provides workers with a "voice" option of resolving problems in the workplace as an alternative to the "exit" option of resolving problems by quitting. Hence, the presence of a union will tend to swing the "exit-voice tradeoff" in favor of the voice option. Note, however, that fringe benefits have not been controlled for in this study, which implies that UNION will contain the effects of the voice option and higher fringes, with both negatively influencing the quit rate.

- PROGRESS < 0. The presence of a wage progression for the job slot signifies that there is future wage improvement with the firm, and hence, future wage increases will be lost if the worker quits. This is consistent with a specific human capital interpretation in which the presence of a wage progression implies that the worker initially invests in human capital by accepting a lower wage and, later, receives a return on investment in the form of higher wages. Under this interpretation, the worker's probability of quitting is inhibited by the prospects of suffering a capital loss--i.e., foregoing higher future wages. In addition, this variable is likely to pick up unmeasured firm characteristics that are correlated with having a wage progression.

- LONGTOPW > 0. The present value of a job in which it takes a longer period of time to reach the top wages is lower than that of a comparable job in which it takes a shorter period of time to reach the top wage, and therefore, the probability of quitting is higher when it takes a longer time to reach the top wage.

- PCTCRAFT < 0. The percent of craft workers in the firm should be highly correlated with job ladders and advancement opportunities and, thus, represents future wage increases for the worker. This variable is a traditional proxy for investment in specific human capital.

- INDUSTRY DUMMIES. There are no priors on the signs of these coefficients since they serve mainly as control variables. Note that the excluded industry is Wholesale and Retail.

Specific Variables

- SCREEN < 0. This measures the intensity of the firm's prehire information gathering process outlined in chapter 2. By investing in prehire information on potential workers, the firm will be better able to hire a worker whose talents are compatible with the job demands, and therefore, is less likely to hire a worker who will become dissatisfied and quit. This screening process may also be indicative of the type of job to be filled and the firm's policy regarding the retention of workers since a firm with jobs that require a great deal of training (either specific or general) will most likely be looking for a relatively permanent employment relationship and, therefore, will attempt to ensure match quality before someone is hired by carefully screening potential workers. In such cases, it may be said that the firm does not draw randomly from the pool of available workers and discovers match quality only after the match has been formed.

- ORIENT < 0. Ideally, this variable captures the effects of on-the-job training--i.e., specific human capital investment. However, the concept of investment in human capital is a long-term notion, and it is difficult to conclude how well the amount of time spent orienting and training the worker during the first month of employment captures this process. At best, ORIENT is a predictor of future training and human capital accumulation. An alternative interpretation that is consistent with a negative sign for ORIENT is that it is an indicator of how well workers become acquainted with the job, and thus, how comfortable they feel in the job.

- WAGE < 0. This follows directly from the worker's search model of Meitzen. A higher wage is more likely to meet the worker's reservation wage, and therefore, the probability of quitting is lower.

Worker Variables

- SEX < 0. As the traditional primary earner of the household, males are less likely to quit their jobs. A selection process may be captured by the coefficient since males, being perceived by employers as having a greater attachment to the labor force than women, will be chosen by firms who are looking for permanent employees.

- AGE < 0. Two factors help to explain this negative sign. First, since their family responsibilities are likely to be fewer and mobility costs lower, job shopping is more likely to be undertaken by younger workers in an attempt to find jobs that suit their tastes or to discover what their tastes are. Second, older workers in the same current position are less likely to switch to a given job than younger workers because their shorter remaining working life means that there is less time for them to capture the returns of a new job. In addition, the number of alternatives available is likely to decline with age. However, it may be argued that AGE is associated with a higher probability of quitting when it is remembered that the EOPP sample contains recently hired workers in low-skilled occupations, and older workers hired in to such positions may be of lower quality or may be

more likely to separate from a job than other members of their age cohort. Therefore, the interpretation of the AGE coefficient may not be as unambiguous as it appears at first glance.

- o SCHOOL > 0. Better educated workers are likely to have better alternative opportunities.

- o PROD2WK < 0. Workers who perform better are likely to have greater future opportunities with the firm. Furthermore, good job performance may be an indication of worker satisfaction with the job.

Market Variables

- o MKTWAGE > 0. Market wage is an indication of the wages paid in alternative jobs. The higher the market wage relative to current wages, the greater the probability that the worker will quit to find a better paying job.

- o DMKTEMPL > 0. The growth in labor market employment is positively correlated with the probability of finding another job. This variable can be thought of as proxying job vacancy rate.

7.3.2 Quit Equation Results

Quit equation results are reported in table 7.9 where the three columns reflect different wage variables included in the equation. Model 3 (the Gompertz hazard) results are presented since, in the previous section, it was judged that there is essentially no difference between Models 1, 2, and 3.

In addition to the coefficient estimates reported in table 7.9, column 1 of table 7.10 presents what are referred to as the quit equation "multipliers." For continuous explanatory variables, the multipliers indicate the proportionate change in the quit rate when the explanatory variable moves from its sample mean to a value one standard deviation above its sampled mean. An interpretation of this continuous variable multiplier is that it represents the proportionate change in the quit rate when the explanatory variable undergoes a "typical" change in the sample. For dummy variables, the multipliers indicate the proportionate change in the quit rate when the explanatory variable moves from a value of 0 to a value of 1. In addition to the multipliers of column 1, table 7.10 reports the sample means, standard deviations, and coefficients of variation for the explanatory variables of the quit equation.

To define the multipliers more formally, first let:

(12) $\underline{X}$ = n-dimensional row vector of explanatory variables.

Partition this vector in the following manner:

(13) $\underline{X} = (\underline{X}_1, X_k)$

TABLE 7.9

QUIT EQUATION WITH ALTERNATIVE WAGE SPECIFICATION

	STARTWAGE Only		TOPWAGE Only		STARTWAGE and TOPWAGE	
CONSTANT	-5.217	(-9.80)	-5.151	(-9.86)	-5.156	(-9.70)
LOGEMPLOY	-.137	(-4.17)	- .142	(-4.29)	- .143	(-4.31)
UNION	-.00517	(-2.30)	- .00471	(-2.10)	- .00478	(-2.13)
PROGRESS	-.532	(-3.95)	- .433	(-3.20)	- .419	(-3.02)
LONGTOPW	-.000759	(-1.29)	- .000364	(- .64)	- .000337	(- .60)
PCTCRAFT	-.836	(-3.44)	- .764	(-3.13)	- .761	(-3.12)
MINING	-.0139	(- .05)	.0413	(.12)	.0128	(.03)
DURABLE	.295	(1.41)	.302	(1.47)	.283	(1.35)
NONDURABLE	-.0872	(- .36)	- .0649	(- .27)	- .0685	(- .28)
FINSERV	-.0607	(- .59)	- .0576	(- .56)	- .0614	(- .60)
TRANSCOM	.297	(1.30)	.345	(1.51)	- .337	(-1.47)
SCREEN	-.0157	(-2.46)	- .0155	(-2.44)	- .0156	(-2.45)
ORIENT	-.00507	(-3.49)	- .00485	(-3.34)	- .00486	(-3.34)
STARTWAGE	-.0757	(-1.93)			.0271	(.42)
TOPWAGE			- .111	(-3.25)	- .123	(-2.73)
SEX	-.0212	(- .22)	.0140	(.15)	.0106	(.11)
AGE	-.0271	(-4.94)	- .0254	(-5.04)	- .0256	(-5.04)
SCHOOL	.0319	(.67)	.0405	(.85)	.0388	(.81)
PROD2WK	-.00824	(-4.05)	- .00837	(-4.12)	- .00842	(-4.14)
MKTWAGE	.0651	(1.07)	.0674	(1.11)	.0665	(1.10)
DMKTEMPL	.564	(.51)	.610	(.56)	.599	(.55)
α	-.0000260	(.09)	- .000004	(0)	.000008	(0)
$\bar{r}_q$	.0006723		.0006723		.0006723	
χ^2	164.62		172.37		172.54	
Log L	-4277.748		-4273.875		-4273.787	

t-statistics in parentheses.

TABLE 7.10

QUIT EQUATION MULTIPLIERS

	Multiplier	$\bar{X}_k$	σ_k	$_k/\bar{X}_k$
LONGEMPLOY	.803	3.027	1.531	.506
UNION	.876	10.36	27.81	2.684
PROGRESS	.658	.883	.322	.365
LONGTOPW	.966	90.46	103.3	1.142
PCTCRAFT	.850	.131	.213	1.635
MINING	1.013	.030	.170	5.667
DURABLE	1.327	.073	.260	3.562
NONDURABLE	.934	.055	.228	4.145
FINSERV	.940	.338	.471	1.393
TRANSCOM	.714	.042	.776	18.476
SCREEN	.635	6.343	29.12	4.591
ORIENT	.840	32.33	35.90	1.110
STARTWAGE	1.036	4.009	1.619	.404
TOPWAGE	.788	5.197	1.933	.372
SEX	1.011	.455	.498	1.095
AGE	.775	27.17	9.980	.367
SCHOOL	1.038	4.166	.965	.232
PROD2WK	.828	55.94	22.47	.402
MKTWAGE	1.053	6.678	.776	.116
DMKTEMPL	1.025	.089	.041	.455

NOTE: Values of β_k are from column 3 of table 4.9.

where

$\underline{X}_1$ = all (n-1) elements of $\underline{X}$ except X_k

X_k = explanatory variable of interest.

Using Model 1 from the quit equation, we have:

(14) $r_q(t|\underline{X}) = e^{\underline{X}\,\underline{\beta}} = e^{\underline{X}_1\underline{\beta}_1}\ e^{X_k\beta_k}$

For continous variables, let:

(15a) $\overline{X}_k$ = sample mean of X_k

(15b) δ_k = standard deviation of X_k

(15c) $\underline{X}'_k$ = value of X_k one standard deviation above its sample mean $= \overline{X}_k + \delta_k$. When X_k is at its sample mean, the quit rate is:

(16) $r_q(t|\underline{X}) = e^{\underline{X}_1\underline{\beta}_1}\ e^{\overline{X}_k\beta_k}$

When X_k is one standa rd deviation above its sample mean, the quit rate is:

(17) $r'_q(t|\underline{X}) = e^{\underline{X}_1\ 1}\ e^{X'_k\beta_k}$

The multiplier for continuous variables indicates the proportionate change in the quit rate when X_k moves from $X_k = \overline{X}_k$ to $X_k = X'_k$:

(18) $$\frac{r'_q}{r_q} = \frac{e^{\underline{X}_1\underline{\beta}_1} \cdot e^{X'_k\beta_k}}{e^{\underline{X}_1\underline{\beta}_1}\ e^{\overline{X}_k\beta_k}}$$

$$= \frac{e^{X'_k\beta_k}}{e^{\overline{X}_k\beta_k}}$$

$$= e^{\beta_k(X'_k - \overline{X}_k)}$$

$$= e^{\beta_k\delta_k}$$

The multiplier for dummy variables indicates the proportionate change in the quit rate when X_k moves from $X_k = 0$ to $X_k = 1$:

(19) $$\frac{r_{q1}}{r_{q0}} = \frac{e^{1\cdot\beta_k}}{e^{0\cdot\beta_k}} = e^{\beta_k}$$

In the following discussion, $1 - e^{\beta_k\delta_k} = \frac{\overline{r}_q - r'_q}{r_q}$ (from equation 17) will be referred to as the percentage change in the quit rate.

Looking at the results for the General variables in column 1 of table 7.9, LOGEMPLOY, UNION, PROGRESS, and PCTCRAFT all have the predicted signs and are statistically significant. Focusing first on LOGEMPLOY and PCTCRAFT, the multipliers of table 7.10 indicate that the probability of quitting is almost 20 percent lower in a firm which is approximately 50 percent larger than the average firm in the sample, while the probability of quitting is 15 percent lower in a firm whose labor force is 34 percent craft workers compared to the sample average of 13 percent craft workers . The results of LOGEMPLOY and PCTCRAFT support the contention that they proxy the existence of internal job ladders where workers are provided training in lower "rungs" of the ladder so that they may eventually fill higher skilled positions in the firm's job hierarchy. Thus, it can be said that the firm creates its own supply of skilled workers and seeks a stable, permanent employment relationship in order to maintain a predictable supply of skilled workers.

The cross-section, industry-level studies of Stoikov and Raimon (1968) and Burton and Parker (1969) generally found firm size to negatively influence the quit rate and, in most cases, the coefficients were not statistically significant. However, in one variant of their model, Burton and Parker found that average firm size in the industry had a positive and statistically significant effect on industry quit rates. Specifically, Burton and Parker found that an increase of 1,000 workers in average firm size would increase the industry quit rate by 12 percent when evaluated at the mean quit rate for the sample. They dismiss this unusual result rather lightly by simply saying that: "The positive relationship between firm size and voluntary mobility contradicts expectations and suggests that earlier research may have inadequately controlled for the impact of other variables on mobility" (p. 213).

Also contrary to the results of this study, Burton and Parker found that the influence of the percent of skilled workers in the industry on industry quit rates was positive, but not statistically significant. However, Stoikov and Raimon found a negative and statistically significant relationship between their "quality of work force" variable and industry quit rates. This quality of work force variable essentially measures the skill-mix of the industry labor force, and it was found that a 1 percent increase in this variable reduces the quit rate by 1.7 percent (where both variables are evaluated at their means).

The negative sign of UNION in table 7.9 supports the exit-voice hypothesis and, in all likelihood, also reflects the effect of additional fringe benefits associated with unionized employment. However, since the level of fringe benefits cannot be controlled for, it is not possible to disentangle the voice effect and the fringe effect of UNION. From table 7.10, it can be seen that the probability of quitting is 12.4 percent lower in a firm which has 38.2 percent of its workers unionized compared to the average firm in the sample which has 10.4 percent of its workers unionized.

In his study, Freeman estimated the union impact on worker quits with four different samples: the National Longitidinal Survey (NLS) of older males; the younger male NLS; the Michigan Panel Survey of Income Dynamics (PSID); and the Current Population Survey (CPS). In all four samples, the

union impact on worker quits was negative with all samples except the younger male NLS that generated statistically significant union coefficients. In addition to not being statistically significant, the union coefficient of the younger male NLS is smaller than the union coefficient of the older male NLS, which leads Freeman to conclude that "unionism has a smaller impact on the exit behavior of younger workers than on the exit behavior of older workers" (p. 665).

In order to compare the union impact on the probability of quitting obtained in this study with Freeman's results, the results of both studies must be translated into roughly equivalent units. The Freeman result chosen for comparison is the logit model of the annual probability of quitting estimated with the PSID data. In the PSID sample, the measure of unionization is the union status of the worker (0 = nonunion, 1 = union member). The NLS samples were not chosen because Freeman does not provide the reader with enough information in order to transform his logit coefficients into percentage changes. The CPS sample was judged to be noncomparable to the EOPP sample because the CPS measure of quits is a worker who has quit and is unemployed. Freeman notes that this is a very special group of workers because quitting and being unemployed is "an extreme form of exit, since most quitters have a job in hand" (p. 662). In fact, in his study, Mattila (1974) concluded that between 50 and 60 percent of quitters line up a new job before they quit.

In a logit model, coefficients of independent variables are transformed into partial derivatives by the following:

$$(20)\quad \frac{\partial Q}{\partial X_i} = \beta_i\, Q(1-Q)$$

where

Q = probability of quitting

β_i = logit coefficient of X_i

X_i = independent variable of interest

After performing this transformation, Freeman (p. 659) reports that for the appropriate model, the impact of unionism on the probability of quitting is 0.036 points when equation 19 is evaluated at the mean quit probability. In order to state this as a percentage change in the quit probability due to unionism, this result should be divided by the mean quit probability. Unfortunately, Freeman does not tell the reader what this mean probability is. Instead, in table 7.2, he reports separate mean quit probabilities for union members (0.058) and nonunion workers (0.090). Using these subsample means reported by Freeman, an approximation to the overall sample mean can be constructed by taking a weighted average of the union and nonunion means with the weights being based on the percent of the labor force that is unionized. Since approximately 20 percent of the labor force is unionized, the constructed mean quit probability is:

$$(21)\quad (.2)(0.058) + (.8)\,(0.090) = 0.0836$$

Therefore, evaluated at the mean quit probability, union members have a 43 percent lower annual quit probability than do nonunion workers in the PSID sample.

Before comparing Freeman's results with the results of this study, it must be noted that the measure of unionization differs between the two studies. As mentioned above, Freeman's measure of unionization is the union status of the worker. However, in the EOPP data, the measure of unionization is the percent of the firm's work force that is unionized. Thus, the PSID data provide information on the worker's union status, without providing information on the degree to which the worker's employer is unionized while the EOPP data provide information on the extent of employer unionization without providing information on the union status of the individual worker. In order to resolve this conflict, it will simply be assumed that in the EOPP sample, workers in highly unionized firms are union members and, therefore, the appropriate comparison is between a union member in the PSID sample and a worker in a "highly unionized" firm in the EOPP sample.

The multipliers calculated from the EOPP sample compare the quit rate in a "highly unionized" firm to the quit rate in firms with no unionization. For example, the multiplier for a firm that is 75 percent unionized is:

$$(22)\quad U_{75} = \frac{r_q(t|X_1,\ \text{UNION} = 75\%)}{r_q(t|X_1,\ \text{UNION} = 0\%)}$$

Multipliers were calculated for 75 percent, 85.8 percent, and 100 percent unionization. The 85.8 percent multiplier was chosen because, for those firms in the EOPP sample reporting unionization over 50 percent, the average percent of the work force unionized was 85.8 percent. These multipliers are: U_{75} = .699; $U_{85.8}$ = .664; U_{100} = .620. Thus, the probability of workers quitting a highly unionized firm is approximately 30 to 38 percent lower than the probability of their quitting a nonunion firm. As in Freeman's findings, these multipliers indicate that unionism has a very large impact on the probability of quitting.

The magnitude of the Freeman union effect may be greater than the union effect found in this study because of the special nature of the EOPP sample. Recall that the EOPP sample consists of recently hired, low skilled workers, and the quit behavior of these workers may be less likely to be affected by unionism than the quit behavior of workers in the more general PSID sample. Furthermore, it must again be emphasized that the results of the two studies may not be strictly comparable due to the difference in the measurement of unionization and also because of the approximation employed here in order to construct Freeman's estimate.

Finally, an issue in the interpretation of UNION is whether the coefficient represents a behavioral response on the part of workers--e.g., the voice option provided by the union lowers the workers' propensity to quit--or whether the coefficient captures the workings of a selection process--e.g., only workers with lower propensities to quit will be hired by unionized firms, and therefore, UNION is indicative of a firm selection policy. Freeman raises

this issue and concludes that in his study, the UNION effect is more likely to be due to a behavioral response:

> Since controlling for individual propensities to quit has essentially no effect on the coefficient of unionism, we conclude that the union impact appears to operate by changing the behavior of the same person rather than by unionization of innately more stable persons. In an organized work place a given individual is less likely to quit than in a non-organized work place, wages held fixed. (p. 666)

The negative coefficient of PROGRESS in table 7.9 is consistent with the interpretation that workers are inhibited from quitting when future wage increases would be foregone. The multiplier in table 7.10 indicates that the probability of quitting is 34.2 percent lower in firms having a wage progression, which is, indeed, a very sizeable reduction. As in the case of interpreting the union coefficient, it must be asked whether PROGRESS represents the workers' behavioral response to the presence of a wage progression--e.g., the propensity to quit is lowered by the presence of a wage progression--or whether the wage progression is a self-selection device by which workers with lower inherent propensities to quit will choose jobs with wage progressions while workers with higher propensities to quit will choose jobs with flat wage profiles. According to Salop and Salop (1976), this self-selection feature of a wage progression operates in the following way:

> The firm discourages high turnover individuals from applying and encourages low turnover workers to apply for employment by predictably increasing an employees' wage with his tenure at the firm. This has the effect of allowing the applicant essentially to guarantee his longevity with the firm, since he himself pays the consequences, in terms of foregone higher earnings, if he quits prematurely. (p. 620)

Unfortunately, the issue of whether PROGRESS represents a behavioral response or a self-selection device is difficult to answer with the EOPP data since no information exists on workers' previous employment history. A clue might come from column 3 of table 7.9 where the inclusion of STARTWAGE, TOPWAGE (the starting wage and the top wage of the job slot), and LONGTOPW essentially hold constant the present value of the job slot. In this case, PROGRESS still has a negative and statistically significant coefficient. Thus, with the present value of the job slot held constant, the presence of a wage progression reduces the quit rate, which lends supporting evidence to the conclusion that a wage progression does function as a self-selection device.

In addition to capturing behavior-modifying and/or self-selection effects of a wage progression, it is possible that PROGRESS is correlated with unmeasured firm characteristics that influence worker quits. For example, if wages are positively correlated with job quality--e.g., working conditions, latitude for worker decision making--then prospects of future wage increases imply that the worker also expects future improvement in nonpecuniary occupational characteristics.

Altnough a variable like PROGRESS was not found in the other studies surveyed, a few roughly comparable variables were found. Parsons' (1973) time series study in which quit rates were estimated for twenty-seven separate industries included a "change in own wage variable." After surveying the results of the twenty-seven regressions, Parsons concluded that this variable has no systematic effect on quits. Stoikov and Raimon included a variable that measured the "percentage increase in earnings over the last three years" in their cross-section study and found that it had a statistically significant negative effect on industry quit rates. They found that a 1 percent increase in this variable reduced industry quit rates by .358 when both independent and dependent variables were evaluated at their means. In terms of relative importance, Stoikov and Raimon found this wage increase variable to have the third largest impact on quit rates behind (1) their measure of quality of the industry work force and (2) the industry wage level.

Even though it is not statistically significant in column 1 of table 7.9, LONGTOPW is negative instead of being positive as predicted. This is largely due to the fact that in column 1, the top wage of the job slot is not controlled for, and since LONGTOPW and TOPWAGE are positively correlated (r = .221), LONGTOPW is partially measuring the effects of higher top wages and the longer time period needed to reach there higher wages. This reasoning is supported by the results in columns 2 and 3 where TOPWAGE is included, and both the size of the coefficient for LONGTOPW and the t-statistic are less than half what they are in column 1.

Focusing on the specific variables in column 1 of table 7.9, SCREEN, ORIENT, and WAGE (i.e., STARTWAGE) all possess the hypothesized signs, with SCREEN and ORIENT being statistically significant at the 5 percent level and WAGE being statistically significant at the 10 percent level. The SCREEN coefficient supports the view that firms can lower the quit propensity of workers by investing in prehire information in order to come up with a good firm-worker match. Under this interpretation, the firm can be thought of as providing an informational service to workers in the sense that workers who are hired by firms which invest in large quantities of screening will find that the probability of the employment match being satisfactory is greater, and, hence, workers will have to invest in less of their own information--e.g., via job shopping.

Table 7.10 reveals that SCREEN has a very large standard deviation and that the value of its coefficient of determination is the largest of any variable in the sample. Not surprisingly, the multiplier for SCREEN is very large as it indicates the probability of quitting is 36.5 percent lower in a firm doing 35.5 hours of screening per opening compared to a firm doing 6.3 hours of screening. This result may be viewed as the difference in quit probabilities between a firm that screens applicants for approximately one work week and a firm that screens applicants for approximately one work day.

Another way to measure the impact of firm screening on worker quit probabilities is to compare the quit rates of firms that provide no screening to firms that provide some positive amount of screening. This is an especially important comparison in the EOPP sample since 29.6 percent of the firms

did no screening. For example, the multiplier for ten hours of screening would be:

$$(23)\quad S_{10} = \frac{r_q(t|X_1,\ \text{SCREEN} = 10)}{r_q(t|X_1,\ \text{SCREEN} = 0)}.$$

This multiplier was calculated for eight, ten, twenty, and forty hours of screening with the results being: $S_8 = .883$; $S_{10} = .856$; $S_{20} = .732$; and $S_{40} = .536$. Thus, compared to a firm doing no screening, a firm doing just one work day of screening reduces the probability of worker quits by 11.7 percent, while a firm doing one work week of screening reduces the quit probability by almost 50 percent.

Although these results seem to be quite significant, care must be taken when interpreting the coefficient of SCREEN and its multipliers for at least two reasons. First, the issue of selectivity again is relevant as it may be argued that the informational screening undertaken by the firm has the primary purpose of identifying workers who have lower inherent propensities to quit and not of finding the best firm-worker match. Second, it must be noted that SCREEN may not be a truly exogenous choice variable as the nature of the firm's occupational structure and the on-the-job training process are likely to determine whether or not the firm is looking for a permanent worker and, hence, whether or not the firm is willing to invest in large amounts of pre-hire information on workers. For example, in cases where the job is relatively simple to learn and mistakes are not costly, the firm may find it cheaper to foresake screening workers before they are hired in favor of randomly drawing workers from the available pool and accepting higher rates of worker turnover.

The negative coefficient of ORIENT in table 7.9 supports the claim that workers who are more familiar with their jobs are less likely to quit. In table 7.10, the multiplier for ORIENT indicates that the probability of quitting is reduced by 16 percent if the firm doubles the amount of time it spends to orient and train the new worker (i.e., increase ORIENT by one standard deviation from its average of 32.33 hours to 68.23 hours). Essentially, this multiplier represents the change in the probability of quitting when the amount of time spent orienting and training the new worker is increased from one work week to two work weeks. In fact, in calculating the multiplier that compares ORIENT = 80 to ORIENT = 40, a 17.7 percent reduction in the probability of quitting is found.

It was mentioned earlier that the relatively short time span covered by this variable (the first month of employment) may not adequately predict a training process which is likely to be spread over a much longer time period, and therefore, it is unclear whether ORIENT can be interpreted as a proxy for specific human capital investment (or, less dogmatically, as a proxy for on-the-job training). A partial answer may be obtained by finding out whether ORIENT can predict future wages since worker training should result in higher future wages for the workers. In table 7.11, for those workers still with the firm at the time of the EOPP interview (N = 2016), the log of their current wage (i.e., the wage they were receiving at the time of the EOPP interview) is

regressed on a number of factors including ORIENT, and it can be seen that ORIENT is positive and statistically significant in predicting the wage. This result is evidence in favor of interpreting ORIENT as a proxy for on-the-job training.

Two studies that are primarily concerned with the effects of specific human capital investment on worker quits are the cross-section industry-level studies of Parsons (1972) and Pencavel (1972). Both use a wide variety of proxies for specific human captial investment, and in general, their results are consistent with their hypotheses. However, just as the true meaning of ORIENT has been questioned in this study, the proxies of Parsons and Pencavel must be viewed with caution because of the seemingly ad hoc manner in which these var- iables are shown to represent specific human capital investment. For example, Pencavel (p. 59) uses the percentage of urban workers in the industry as a proxy for specific human capital investment "on the conjecture that urban and rural workers contain differing amounts of specific training."

STARTWAGE in column 1 of table 7.9 is the starting wage at which the worker was hired, and its sign is consistent with the view that workers are less likely to quit higher paying jobs. However, in accord with the job matching model of chapter 2 of Meitzen, once the worker has been hired, future wages with the firm are the primary consideration in the worker's quit decision. To remedy this, column 2 replaces STARTWAGE with TOPWAGE, the top wage for the job slot, and it has a large and significant negative effect on the quit rate. Column 3 includes both STARTWAGE and TOPWAGE, and a comparison of the log likelihoods for columns 2 and 3 shows them to be virtually identical, and, thus, adding STARTWAGE to the model that already includes TOPWAGE adds no explanatory power to the model. This is because of the high degree of collinearity between STARTWAGE and TOPWAGE ($r = .710$), which implies that STARTWAGE in column 1 is a proxy for TOPWAGE. Notice in column 3 that when STARTWAGE is included, the coefficient of TOPWAGE is similar to what it was in column 2 but, even though it is not statistically significant, the coefficient of STARTWAGE becomes positive compared to a negative coefficient in column 1. Actually, this is not as startling as it may first appear because in column 3, where TOPWAGE is held constant, a higher STARTWAGE implies that the wage profile for the job is flatter, and hence, the worker has less future wage increases to look forward to. The flatter wage profile implied by the higher value of STARTWAGE also means that workers are paying for a smaller share of any human capital investment, and therefore, suffer a smaller capital loss if they quit.

When speaking of the worker's decision to quit in response to low wages, it should be kept in mind that firms are not necessarily passive agents in this decision since they may try to raise wages if they are concerned about worker quits. In other words, the worker's decision to quit requires tacit approval by the firm.

In table 7.10, the multiplier for TOPWAGE shows that a 37 percent increase in the top wage for the job slot (from \$5.20 to \$7.13) reduces the probability of quitting by just over 21 percent. Calculating the multiplier for a \$1.00 increase, the probability of quitting declines 11.6 percent for

every $1.00 increase in the top wage. Evaluated at the mean of TOPWAGE, this implies that a 1 percent increase in TOPWAGE leads to a .6 percent decrease in the probability of quitting.

Many of the studies surveyed included a wage variable in their quit equations. In their cross-section studies, Burton and Parker, Parsons (1972), Pencavel, and Stoikov and Raimon all found statistically significant negative relationships between wages and industry quit rates. The only researchers who reported their results in elasticity terms were Stoikov and Raimon who found that a 1 percent change in "average annual earnings adjusted for quality of the work force" lead to a -1.075 change in industry quit rates.

In their longitudinal studies, Bartel (1980) and Freeman (1980) also reported negative wage coefficients. Using a probit model, Bartel estimated the effect of the logarithm of wages on the probability of quitting for both the NLS younger male and NLS older male samples. In both samples, statistically significant negative results were obtained. However, Bartel failed to provide enough information in order to transform these coefficients into elasticities. In the PSID sample, Freeman reported a statistically significant logit coefficient for the logarithm of wages. Again, not enough information was provided in order to transform this result into a magnitude that is comparable to the results of this study.

Focusing on the worker variables in table 7.9, all have the hypothesized signs, but only AGE and PROD2WK are statistically significant. The AGE multiplier in table 7.10 indicates that a worker thirty-seven years of age has a 22.5 percent lower probability of quitting than a worker who is twenty-seven years old. More generally, every ten years of age results in a 22.5 percent reduction in the probability of quitting. Mincer and Jovanovic (1979) spell out two reasons for the negative coefficient of AGE that have also been mentioned here. First, older workers have a shorter period in which to reap the benefits of a higher paying job:

> For a given wage gain, the supply response would diminish with working age (at given levels of tenure) since the payoff period declines. (pp. 24-25)

Second, younger workers are more likely to be engaged in job shopping.

> Another set of age factors, unrelated to location, may operate in the early years of work experience. The range of quality of jobs and of the job match cannot be ascertained by mere search, and some knowledge must be acquired by actual experimentation. Also, job training and opportunities for investment in general human capital may present themselves sequentially in different firms. Beyond the first decade of working life, we may expect that human capital investors who eventually find a reasonably compatible work place develop a strong attachment to the job. (pp. 24-25)

TABLE 7.11

CURRENT WAGE EQUATION

CONSTANT	.383	(5.18)
LONGEMPLOY	.0176	(3.89)
UNION	.00155	(6.46)
PCTSEP	-.0287	(-2.58)
MINING	.533	(14.90)
DURABLE	.285	(10.22)
NONDURABLE	.137	(4.78)
FINSERV	.0679	(4.55)
TRANSCOM	.222	(6.94)
PCTCRAFT	.155	(5.27)
ORIENT	.000631	(3.60)
SCREEN	.000191	(.79)
LONGTOPW	.000207	(3.28)
TENURE	.000192	(5.06)
SEX	.184	(13.83)
AGE	.00267	(4.35)
SCHOOL	.0762	(11.51)
PROD2WK	.000181	(.62)
MKTWAGE	.0388	(4.68)
DMKTEMPL	.476	(2.94)

Dependent Variable: Log of Current Wage

N = 2016

R^2 = .3805

t-statistics in parentheses.

Freeman finds a negative and statistically significant relationship between age and the annual probability of quitting in the PSID sample. His logit coefficient approximately translates into a 3.6 percent reduction in the probability of quitting for each year of age when the probability of quitting is evaluated at its mean. This is somewhat larger than the reduction found in this study, which is approximately 2.3 percent for each year of age. Mincer and Jovanovic also use the PSID sample in a linear regression where the

"annual job change rate" is the dependent variable. They find that one year of "work experience" reduces the job change rate by 5.3 percent at the mean value of the job change rate.

The negative coefficient of PROD2WK in table 7.9 may be interpreted as implying that workers who are performing better are more likely to stay on the job since their performance is an indicator of job satisfaction. However, this interpretation is suspect for two reasons. First, how well does productivity in the second week of employment predict workers' future productivity? This is essentially the same problem that was discussed in connection with the predictive power of ORIENT. Assuming that productivity and wages are positively correlated, looking at table 7.10, it can be seen that the coefficient of PROD2WK is small and not statistically significant. Therefore, PROD2WK is not a good predictor of future productivity or of future wages. Secondly, given these results, it must be asked what PROD2WK is actually measuring. Recall that this question in the EOPP survey is answered retrospectively by the employer, and therefore, this variable may be capturing nothing more than the firm's satisfaction with the job match. Workers who stay are more likely to be judged favorably than workers who leave since it is possible that workers who are still with the firm are there because of mutual satisfaction with the match. Hence, it may be more accurate to call PROD2WK "satisfaction with the match."

The rather poor showing of SCHOOL in this study is consistent with the results of other studies. In the studies surveyed that included an education variable (Bartel, Freeman, Mincer and Jovonovic, Parsons and Pencavel (1972)), the sign of the education variable was both positive and negative and was usually not statistically significant. In fact, in only one case, the Mincer and Jovanovic young male NLS sample, was the coefficient statistically significant. In this case the coefficient was negative.

Similarly, the studies that included a sex variable [Burton and Parker, Parsons (1972) and Stoikov and Raimon] did not generally come up with good results. The percent of employees that are female had both positive and negative effects on quit rates. Again, statistical significance was not very common, with only Burton and Parker finding that the percent of male workers in the industry had a statistically significant negative effect on the industry quit rate.

Focusing on the market variables, both MKTWAGE and DMKTEMPL have the hypothesized signs in table 7.9, but are not statistically significant. Hence, these results support somewhat the assertion that workers are more likely to quit when market conditions are favorable--i.e., when wages are high and jobs are relatively easy to find. The multiplier for MKTWAGE in table 7.10 suggests that a $.78 rise in the average market wage rate will result in a 5.3 percent higher probability of quitting. Evaluated at the sample mean of MKTWAGE, this translates into an elasticity of approximately .46. The multiplier for DMKTEMPL is very small, as it indicates that a 45 percent increase in the percentage change in market employment results in only a 2.5 percent increase in the probability of quitting--an elasticity of .05 when evaluated at the mean of DMKTEMPL.

In his time series study, Parsons (1973) included variables analogous to MKTWAGE and DMKTEMPL and came up with essentially the same results. He found relative wages, defined as the industry's wage rate divided by the all-manufacturing average wage, generally to be related negatively to the industry's quit rates, but the performance of variables was judged by Parsons to be erratic. The sign of Parsons' variable is consistent with the positive sign of MKTWAGE in this study since, ceteris paribus, a higher market wage implies a lower relative wage for the worker. Parsons found his vacancy rate measure to be positively related to quit rates and, in general, statistically significant. This is consistent with the sign of DMKTEMPL in this study since DMKTEMPL is attempting to proxy the availability of alternative jobs. However, unlike the rather poor performance of DMKTEMPL in this study, Parsons found vacancy rates to be a major factor in explaining variation in industrial quit rates. Parsons (p. 398) reports that the model quit rate-vacancy rate elasticities usually lie in the range of 1.0 to 2.0 for the twenty-seven industries studied.

Turning attention to the duration coefficient, σ, it is noted that in all three columns of table 7.9, σ is very small and not statistically significant. In fact, when the highly significant variable, TOPWAGE, is added in columns 2 and 3, essentially vanishes! This supports the view that duration dependence is due, in part, to uncorrected sample heterogeneity. Aside from this reason, why is there no duration dependence evident in the quit equation? A plausible explanation is that it is due to the short period of observation allowed by the EOPP sample design. At most, a worker's employment with the firm can be viewed for two and one-half years--e.g., a worker hired during January 1978 in a firm interviewed during May 1980--while the downward sloping tenure-turnover profile is a long-term concept where the turnover-inhibiting effects of specific human capital accumulation and wage growth only come into effect after a number of years on the job. In fact, the short time period and the resulting flat tenure-turnover profile implied by this study are consistent with the view that the early part of the employment relationship is a learning period in which bad matches are terminated, and hence, the profile is flat or upward sloping but it eventually declines as learning is completed and the mobility-inhibiting effects of specific human capital accumulation come into focus.

In their analysis of transitions from employment to nonemployment, Flinn and Heckman (1980, pp. 55-56) also found a flat tenure-turnover profile--i.e., no duration dependence of the hazard. However, in their study of mobility over a ten-year period, Mincer and Jovanovic found a negative relationship between worker mobility and job tenure. Furthermore, when they include a variable measuring prior mobility by the worker, the tenure effect diminishes because prior mobility partially corrects for heterogeneity bias in the tenure coefficient.

7.4 Interpretation of the Discharge Results

Two basic types of factors provide guidance for developing hypotheses regarding the signs of the discharge equation coefficients: factors that

determine the quality of the match and factors that contribute to the firm's cost of discharging a worker. Quality of the match variables includes worker characteristics and the amount of information on the match the firm has prior to making the match. Firm cost of discharge factors includes the amount of fixed costs the firm has invested in the worker (which become a capital loss if the worker leaves), the costs of hiring a replacement worker (which can be predicted by the amount of fixed costs the firm has invested in the current worker), and the costs of the actual discharge process. Notice that quality factors and costs of discharge tend to be interrelated as the firm's investment in the worker presumably increases the quality of the worker via increases in worker productivity.

7.4.1 Discharge Equation Hypotheses

General Variables

- LOGEMPLOY < 0. Since firm size represents the degree of internal labor market development, larger firms will be looking for a more permanent employment relationship, and therefore, will be more intent upon coming up with a good match in the hiring process--i.e., firm size is a proxy for th quality of prehire screening. However, countering this effect may be the fact that in larger organizations, relationships tend to be less personal and governed more by rules and regulations, and therefore, a worker may be more likely to be discharged.

- UNION < 0. Firms are less able to take unilateral action in personnel matters when a collective bargaining agreement is in place since formal channels specified by the agreement must be followed, and therefore, it is more costly for the firm to discharge a worker. Furthermore, since the firm is aware of the fact that discharges are costly, it will be more careful in the hiring process in order to increase the probability of forming a good employment match.

  The union effect on the firm's decision to discharge a worker may be different at various stages of the worker's career with the firm. Specifically, the initial stage of employment is usually a trial or probationary period during which the worker is not fully covered by the collective bargaining agreement. Only after the probationary period is passed does the worker come under full protection of the union contract. Therefore, the firm has more latitude in its discharge decision during the probation-probationary period--i.e., it is less costly to discharge a worker who is not fully protected by the collective bargaining agreement. Under these circumstances it may be argued that the real union impact on reducing the probability of discharge does not occur until the probationary period has expired. In fact, it seems reasonable to hypothesize that the sign of the union coefficient is positive during the worker's probationary period since the firm will want to discharge those perceived as poor workers when the costs of doing so are relatively low.

- PROGRESS > 0. In the specific human capital framework, a wage progression implies that workers pay a portion of the fixed employment costs, and therefore, the firm can discharge a worker without bearing the entire capital loss of such a separation.

- LONGTOPW < 0. The longer it takes to reach the top wage of the job slot, the longer workers have to "prove" themselves, and therefore, the less likely they are to be discharged.

- PCTCRAFT < 0. This is a proxy for internal labor market development and, hence, a proxy for the quality of prehire screening.

Specific Variables

- SCREEN < 0. The more the firm invests in prehire screening, the more likely it is that a good match will be formed.

- ORIENT < 0. This proxy for firm investment in specific capital is correlated with the capital loss suffered (or the costs of replacing the worker) if the worker is discharged.

- WAGE > 0. The higher the wage paid by the firm, the more demanding the firm will be regarding worker performance and, therefore, the more likely it is that a given worker performance level will not meet the firm's standards.

Worker Variables

- SEX. There is no a priori reason for expecting either a positive or negative sign on this coefficient. Possibly, if sex discrimination were present, the coefficient would be negative.

- AGE < 0. Assuming age is a proxy for worker quality.

  SCHOOL < 0. An indicator of worker quality.

- PROD2WK < 0. Assuming productivity in the second week on the job is a good predictor of future productivity.

Market Variables

- MKTWAGE > 0. The higher the market wage relative to the firm's wage, the lower the quality of worker the firm can obtain.

- DMKTEMPL < 0. Faster growing labor markets are generally also tighter markets--i.e., demand greater than supply. Firms should be more reluctant to discharge workers in fast growing labor markets since replacements are harder and more costly to find.

7.4.2 Discharge Equation Results

The results for the Model 3 version of the discharge equation with alternative wage specifications are found in table 7.12. Recall from the previous section that a large and significant omitted variable effect was detected in the discharge equation and that the coefficients of Model 3 are generally smaller in absolute value than the coefficients of Model 2 in which the omitted variable effect is accounted for. However, given the overall poor performance (in column 1, only three variables other than the constant are statistically significant), the downward bias in the coefficients of Model 3 is not a very serious problem. Because of the poor results, multipliers were not calculated for the discharge equation.

Although they all possess the hypothesized signs, the only general variable that is statistically significant at 5 percent is LONGTOPW. In fact, a likelihood ratio test of the hypothesis that, as a group, the general variables are not statistically different from zero (i.e., H_0: $\underline{\beta}_G = \underline{0}$) cannot be rejected at the 5 percent level as the value of the likelihood ratio is 11.5 and the critical value of the chi-square statistic at 5 percent with ten restrictions is 18.31. Thus, as a group, general firm characteristics do not contribute to explaining worker discharges.

Even though it is not statistically significant, the UNION coefficient merits further investigation to determine whether or not unionization has a differential impact on the firm's propensity to discharge workers who are in their probationary period and workers who have passed their probationary period. In order to do this, the worker's spell of employment with the firm is segmented into two periods--the first ninety days of employment and the period of employment after the first ninety days--and the effect of UNION on the rate of discharge is estimated separately for the two periods. The first ninety-day period of employment with the firm is interpreted as the worker's probationary period, but, of course, this is not a universal probationary period as some are longer and others are shorter.

The top panel of table 7.13 presents the UNION coefficients for this time period model (all other coefficients are essentially the same as before) with UNION1 being the union effect on the rate of discharge during the first ninety days of employment, and UNION2 being the union effect on the rate of discharge for the period after the first ninety days of employment. Inspection reveals that neither UNION1 nor UNION2 is statistically significant and that UNION1 is approximately ten times as large as UNION2. This seems to indicate that any union effect on the discharge rate is concentrated in the initial period of employment, possibly due to the fact that unionized employers are more careful in selecting new workers and, therefore, are less likely to discharge a worker because of a mismatch. However, this interpretation must be tempered by the fact that UNION1 and UNION2 are not statistically different from one another. This conclusion is reached by performing a likelihood ratio test with the null hypothesis that UNION1 and UNION2 are equal. The unrestricted model--i.e., where UNION1 and UNION2 are not constrained to be equal--is found in the top panel of table 7.13, while the restricted model--i.e., where UNION1 and UNION2

TABLE 7.12

DISCHARGE EQUATION WITH ALTERNATIVE WAGE SPECIFICATIONS

CONSTANT	-8.333	(-6.18)	-8.316	(-6.18)	-8.331	(-6.18)
LOGEMPLOY	- .0724	(- .92)	- .0708	(.91)	- .0728	(- .93)
UNION	- .00239	(- .49)	- .00221	(- .45)	- .00235	(- .48)
PROGRESS	.565	(1.30)	.551	(1.28)	.571	(1.31)
LONGTOPW	- .00387	(-1.97)	- .00384	(-1.89)	- .00380	(-1.86)
PCTCRAFT	- .108	(- .24)	- .102	(- .22)	- .0993	(- .21)
MINING	- .181	(- .27)	- .129	(- .21)	- .179	(- .27)
DURABLE	- .587	(-1.15)	- .563	(-1.12)	- .589	(-1.15)
NONDURABLE	-1.132	(-1.51)	-1.118	(-1.50)	-1.132	(-1.51)
FINSERV	.0751	(.31)	.0786	(.33)	.0747	(.31)
TRANSCOM	- .589	(- .80)	- .570	(- .78)	- .586	(- .79)
SCREEN	- .00386	(- .37)	- .00372	(- .36)	- .00388	(- .37)
ORIENT	- .0000772	(- .03)	- .0000233	(0)	- .0000503	(0)
STARTWAGE	.0209	(.24)			.0289	(.26)
TOPWAGE			.00265	(.05)	- .00977	(- .12)
SEX	1.033	(4.24)	1.043	(4.27)	1.037	(4.23)
AGE	.00993	(1.03)	.0102	(1.06)	.00986	(1.02)
SCHOOL	- .162	(-1.38)	- .156	(-1.35)	- .161	(-1.37)
PROD2WK	- .0282	(-5.61)	- .0282	(-5.60)	- .0282	(-5.61)
MKTWAGE	.114	(.76)	.116	(.77)	.114	(.77)
DMKTEMPL	.369	(.14)	.379	(.15)	.371	(.15)
α	- .00138	(-1.80)	- .00139	(-1.80)	- .00138	(-1.79)
$\bar{r}$	.0001152		.0001152		.0001152	
x^2	77.99		77.94		78.00	
Log L	-867.167		-867.194		-867.159	

t-statistics in parentheses.

TABLE 7.13

DIFFERENTIAL UNION EFFECTS ON THE RATE OF DISCHARGE

UNRESTRICTED MODEL:		
UNION1	-.00620	(-.73)
UNION2	-.000599	(-.11)
Log of Likelihood	-867.701	
RESTRICTED MODEL: (UNION1 = UNION2)		
UNION	-.00248	(-.50)
Log of Likelihood	-867.878	

are constrained to be equal--is found in the bottom panel of table 7.13. Taking the unrestricted and restricted log likelihoods and constructing the likelihood ratio gives a value of .354 while the critical chi-square value at the 5 percent level of significant is 3.841. Therefore, the null hypothesis cannot be rejected and, statistically speaking, there are no differential union effects between workers who are in their probationary period and workers who have passed their probationary period on the probability of being discharged.

Returning to table 7.12, none of the specific variables are statistically significant, and again, a likelihood ratio test of the null hypothesis that, as a group, the specific variables are not significantly different from zero (i.e., H_0: $\underline{\beta_S} = \underline{0}$) cannot be rejected at the 5 percent level as the value of the likelihood ratio is 5.6 and the critical value of the chi-square statistic with four restrictions is 9.49. Although their t-statistics are very low, note that in columns 1 and 2 when STARTWAGE and TOPWAGE are entered into the equation individually, both are positive as hypothesized. However, in column 3 where both are included, STARTWAGE is positive and TOPWAGE is negative. This can be explained from a specific human capital framework by noting that when TOPWAGE is controlled for, a rising STARTWAGE implies a flatter wage profile, and therefore, a larger firm investment in fixed employment costs, which implies that the firm will be less likely to let a worker go.

On the other hand, controlling for STARTWAGE, a higher TOPWAGE represents a steeper wage profile, which implies less capital investment by the firm and, therefore, a higher probability of discharge.

The set of worker variables contains two statistically significant coefficients: SEX and PROD2WK. As mentioned previously, there is no a priori reason to expect that males are more likely to be discharged than females. One reason for this result may be that SEX is highly correlated with unobserved occupational or firm characteristics which are correlated with the probability

of discharge. For example, it may be that more men in the sample hold jobs in the "secondary" labor market where employment is less stable, and hence, discharges are more likely to occur. Again, it must be questioned whether PROD2WK is a measure of actual job performance or whether it represents the firm's subjective judgment about the worker. Recall that the results of table 7.10 in which PROD2WK did not predict wages lend evidence to the claim that PROD2WK is more likely to be an indicator of the firm's satisfaction with the match rather than an objective performance standard. In fact, it makes a great deal of sense to assume that workers who were discharged were thought of very unfavorably by the firms, and hence, would automatically receive a very low rating on this question.

Neither of the market variables is statistically significant, with MKTWAGE possessing the hypothesized sign and DMKTEMPL having a sign opposite that which was hypothesized.

Unlike the quit equation, the duration dependence coefficient, α, is highly visible in the discharge equation. However, interpreting as representing structural duration dependence--e.g., the effect of the match learning process--appears to be unwarranted since the results of Model 2 in the previous section suggest that the observed duration dependence is largely due to uncorrected sample heterogeneity. For example, the omitted variable term, v, of Model 2 may be capturing the effects of an unmeasured variable such as firm discharge policy. Suppose there are two types of manager, A and B, and that Manager A's personnel policy is "I will not tolerate incompetence," while Manager B's personnel policy is "Everyone makes mistakes and I don't hold it against them." Thus, ceteris paribus, a worker is more likely to be discharged by Manager A than by Manager B, i.e., Manager A has a higher propensity to discharge. In Model 2, the average propensity to discharge at a given point in time (i.e., employment duration) is captured by the average of the omitted variable effect, $\bar{v}$. As duration increases, fewer A-type managers will be in the sample as they are more likely to discharge workers, and therefore, $\bar{v}$ will fall resulting in an apparent negative duration dependence of the hazard.

In sum, the discharge results are generally poor, with only three variables being statistically significant. One interpretation of our results is that a discharge is a random, spur-of-the-moment event brought on by very special circumstances that surround the event. However, a probit model of involuntary separations that is reported in chapter 8 of this monograph does much better, so the problem may be in the specification--specifically the lack of controls for job characteristics.

7.5 Summary

This chapter has presented the empirical analysis of the determinants of worker quits and discharges. Section 7.1 provided a description of the sample and the variables used in the study. The sample consists of recently hired low and semiskilled workers in firms interviewed by the Employment Opportunities Pilot Programs (EOPP) Employer Survey. The EOPP sample design only

allowed for the observation of workers during their initial stages of employment with the firm; at most, the worker was observed for only the first two and one-half years of employment. This creates right-censored observations since the entire spell of employment with the firm is not observed for workers who are still employed at the time the firm is interviewed by EOPP.

The variables used in the sample consist of firm characteristics, worker characteristics, and local labor market conditions. Firm characteristics can be further subdivided into general firm characteristics, which are not directly related to the newly hired worker--e.g., size of firm and industry--and specific firm characteristics, which are directly related to the newly hired worker--e.g., the wage paid to the worker and the time spent orienting and training the worker.

In section 7.2, the econometric properties of the preferred maximum likelihood technique were presented. First, the maximum likelihood technique, which is able to adjust for right-censoring by having the incomplete employment spells contribute a "probability of no separation occurring" term to the likelihood, was compared to least squares techniques. The use of least squares when right-censored observations are present requires the imposition of a sample selection rule in order to deal with censored observations. The maximum likelihood technique was compared to least squares under two alternative sample selection rules: (1) use only completed employment spells and (2) include incomplete spells and treat them as if they were complete at the end of the observation period. With the EOPP data, it was found that least squares under both sample selection rules produced coefficient estimates that were closer to zero than the maximum likelihood procedure.

Second, the effects of uncorrected sample heterogeneity on the coefficient estimates and on measured duration dependence were discussed. A significant degree of uncorrected sample heterogeneity was detected in the discharge equation, while virtually none was detected in the quit equation. When comparing coefficient estimates of econometric models where sample heterogeneity was left uncorrected to coefficient estimates of models where a correction was made, it was found that uncorrected sample heterogeneity produced downwardly biased coefficients in the discharge equation and had no effect on coefficient estimates in the quit equation. It was also found that sample heterogeneity "predicted" measured duration dependence as the duration coefficient was significantly negative in the discharge equation and was virtually zero in the quit equation.

Finally, it was pointed out that the EOPP sampling design may result in choosing employment spells that are longer than spells chosen at random. The effect of this length-bias problem would be coefficient estimates that are upwardly biased, compared to coefficients produced from a random sample of employment spells.

Section 7.3 presented an interpretation of the our results. The quit equation featured an extensive discussion of the impact of unionism on worker quits. Supporting evidence was found for Freeman's "exit-voice" hypothesis, which states that unionism inhibits worker quits by providing the worker with

a "voice" option for resolving problems in the workplace as an alternative to resolving problems by quitting. In addition, it was found that the existence of internal job ladders, with their associated on-the-job training and prospects of future opportunities with the firm, inhibited worker quits; that prehire information gathering by the firm resulted in better employment matches being formed, and hence, in a lower quit probability; older workers were less likely to quit; and that workers responded to market forces in the form of relative wages and the availability of other job opportunities in their job mobility decisions. As mentioned previously, the probability of quitting did not vary with tenure of the worker. A possible reason why no duration dependence was observed for the quit equation is the relatively short time horizon imposed by the EOPP sample design: at most, the worker was observed for only the first two and one-half years of employment with the firm. Thus, the mobility-inhibiting effects of factors such as firm-specific skills and seniority-related benefits that come into focus later in the worker's career with the firm may not be captured in the EOPP sample.

The discharge equation reported in table 7.4 was not very satisfactory. Either the model suffers from specification bias or there is no systematic model that can predict these very random events. The significant duration dependence of discharge was found to be the result of uncorrected sample heterogeneity.

NOTES

1. See footnote 1 to chapter 3.

2. See Appendix B for a discussion of the length-bias problem.

APPENDIX 7A

Explanation of Terms and Conventions Used

This appendix contains the definitions of some of the terms used and some of the conventions followed in the tables.

1. T-statistics are reported in parentheses below the coefficient estimates.

2. Log L is the log of the likelihood function.

3. The chi-square statistics reported are for the likelihood ratio tests used to determine whether or not the entire set of coefficients is statistically different from zero. Partition $\underline{\beta}$ in the following manner:

$$\underline{\beta} = \begin{pmatrix} \text{constant} \\ \beta_1 \end{pmatrix}$$

and L is defined as the unrestricted log of the likelihood (i.e., when the full set of variables is used--$\beta_1 = 0$) and L_0 is define as the restricted log of the likelihood, (i.e., only a constant term is used--$\beta_1 = 0$). The null hypothesis being tested is:

$$H_0: \ \beta_1 = \underline{0}.$$

$\lambda = 2[L - L_0]$ is distributed chi-square with n degrees of freedom where n is equal to the number of restrictions imposed--i.e., the number of elements in β_1. If λ is less than the critical chi-square value at the selected level of significance, the null hypothesis cannot be rejected. If λ is greater than the critical chi-square value, the null hypothesis is rejected.

4. $\bar{r}_d$ and $\bar{r}_q$ are the constant rates of separation--i.e., evaluated at $\beta = \underline{0}$. The time unit is in days, and therefore, multiplying by 30 will give the monthly probability of separation.

APPENDIX 7B

The Length-Bias Problem

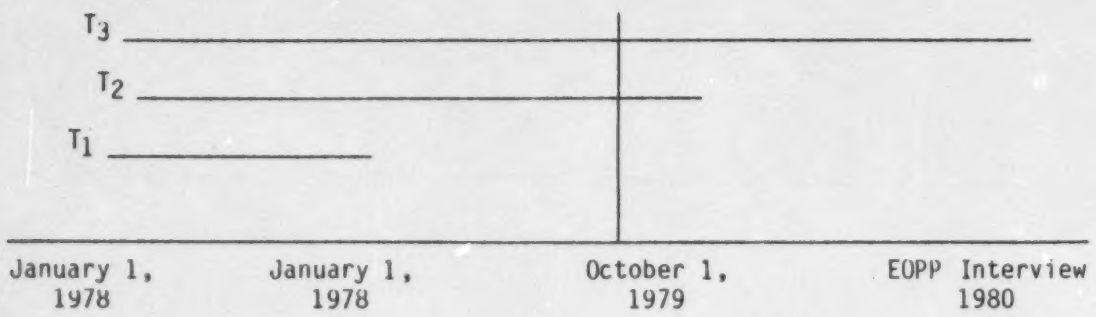
Employment spells contained in the EOPP sample may be longer than employment spells chosen at random. This length-bias problem is due to the nonrandom sampling technique used in the EOPP Survey that fixes a point in time (October 1, 1979) before which the employment spell included in the survey must have begun--i.e., the last worker hired by the firm must have been hired on or before October 1, 1979. As shown in Figure 1, three types of employment spells are generated by this procedure:

T_1: Spells completed on or before October 1, 1979.

T_2: Spells in process on October 1, 1979, which were completed on or before the EOPP interview in the spring of 1980.

T_3: Spells in process on October 1, 1979, which were not completed before the EOPP interview.

FIGURE 1



(Since T_1 spells only account for 6 percent of the EOPP sample, attention will be focused on T_2 and T_3 spells--i.e., spells in process on October 1, 1979). Intuitively, given the fixed sampling point, longer spells are more likely to cover this point than shorter spells, and hence, the employment spells generated by this scheme are likely to be longer than spells that are just chosen at random.

Feller (1971, pp. 11-14) illustrates this problem by assuming that spells or "waiting times" (in the present context, the waiting time until the end of the employment spell) are exponentially distributed:

(1) $f(t) = \alpha e^{\alpha t}$

(2) $E(t) = T = \alpha^{-1}$

Thus, under the exponential assumption, random sampling of employment spells leads one to expect that a spell of length α^{-1} will be chosen. However, Feller (p. 13) states that when requiring selected spells to cover a fixed point, spells so chosen will have an expectation of $2\alpha^{-1}$. Again, the intuition is that longer spells are more likely to cover the fixed point than shorter spells.

CHAPTER 8

PRODUCTIVITY GROWTH AND TURNOVER

John H. Bishop

8.1 Introduction

Studies have consistently found that there is a strong positive correlation between a worker's tenure with a firm and that individual's wage rate. Becker's (1975) on-the-job training (OJT) model is the most widely accepted explanation for this association. The OJT model posits that new employees receive training early in their tenure, which raises their productivity both in and outside the firm. Competition forces the employer to pay employees who have completed this training at least as much as they are worth outside the firm. Jobs that offer such training are more attractive than jobs that do not, so competition forces down the entry wage of jobs that provide training below the entry wage of jobs that offer no training. During the training period, the supervisors and other workers are spending time away from other activities, helping the new employee learn the job. The employer must be compensated for the resulting sacrifice in current output. When the training provides general skills, the only way such compensation can be provided is by a further lowering of the entry wage. Thus, there are two forces that cause wage rates of new employees to rise: the increase of the employee's productivity and the decline of training expenses. When training is entirely specific, and therefore does not raise the worker's productivity in other firms, the forces causing a rising wage profile are weaker. They do not disappear, however, for a rising wage profile reduces the quit rate of trained workers, and thus protects the firm's investment in training.

Recently a number of papers have proposed alternate explanations of the positive correlation between tenure and wage rates. Salop and Salop (1976) have proposed that a rising wage profile may be a strategy for attracting workers with low quit propensities to a firm. Jovanovic (1979) has developed a job-matching theory of turnover which hypothesizes that workers remain in jobs in which their productivity is high and are fired (or quit) from jobs in which their productivity is low. He concludes that "since wages always equal expected marginal products for all workers, the model generates (an average) wage growth as tenure increases." (p. 974) A rising wage profile has also been shown to be a consequence of efforts to prevent shirking (Lazear 1981). Lazear and Moore tested this model and concluded that "under some strong assumptions, our conclusion . . . is that most of the slope of the age earnings profile reflects incentive based wealth and not human capital accumulation via on-the-job training." (Lazear and Moore 1981, p. 19)

The most persuasive attack on the dominance of the OJT explanation for wage growth comes from a series of papers by Medoff (1977) and by Medoff and Abraham (1980, 1981a, 1981b). Medoff and Abraham observed that "despite the straightforward nature of the test required to establish empirically the

superiority of the human capital explanation of the experience-earnings profile over alternative models, . . . no one has ever provided evidence which demonstrates that experience-earnings differentials can in fact be explained by experience-productivity differentials" (1981a, p. 187). Using microdata from the personnel records of four large United States corporations, Medoff and Abraham found that while within a grade level there is a positive association between wage rate and experience, there is a negative association between performance rating and experience. They concluded that "under the assumption that rated performance is a valid indicator of relative productivity, our results imply that a substantial fraction of the return to experience among the groups we are studying is unrelated to productivity" (1981a, p. 187). Medoff and Abraham also reviewed a large number of other studies and concluded that employees with less-than-average senority who are beyond the initial very short orientation/training period are normally slightly more productive than workers with more than average senority (Medoff and Abraham 1981a). Most of the studies reviewed were either of unionized employees or of employees in large corporations.

In this chapter we will replicate the tests conducted by Medoff and Abraham on a very different sample of workers: newly hired, predominantly low skill workers in small- and medium-sized establishments. We hypothesize that in this labor market, significant improvements in productivity occur in the first months and years of employment and that OJT is responsible for much of the productivity gain. Another purpose of the paper is to measure the extent to which the patterns of wage and productivity growth can be explained by human capital theory. In this chapter and the chapter to follow, we will test the following implications of human capital theory.

a. The rate of growth of a worker's current contribution to output (productivity) in the first year of employment depends positively upon the amount of training the worker receives.

b. The growth of a worker's productivity does not depend on what share of the training is general and what share is specific.

c. The wage growth depends positively upon productivity growth.

d. Holding constant the growth of productivity, the growth of the worker's wage rate depends positively on the share of training that produces general as opposed to specific skills.

e. Holding constant the growth of productivity and the share of training that is general, wage growth depends positively on the accuracy of performance measures and the acceptability of basing raises on these performance measures (Hashimoto and Yu 1980).

f. The greater the relative risk of a quit, the greater will be the share of specific investment paid for by the employee, and therefore, the greater will be the rate of wage growth (Hashimoto 1981a).

g. The greater the responsiveness of the quit rate to the posttraining wage, the greater will be the rate of wage growth (Hashimoto 1981a).

h. The greater the relative risk of being fired and the responsiveness of this risk to the posttraining wage rate, the smaller will be the rate of wage growth (Hashimoto 1981a).

These chapters will also examine Jovanovic's sorting/selection explanation of the increase in average productivity and wage rates with tenure. Sorting/selection can cause a significant rise in wage rates and productivity only if the less productive workers are considerably more likely to separate from the firm than the productive workers. We will test the strength of this relationship by measuring the degree to which turnover selects out the poor performers and by calculating how much of the general rise in average productivity with early tenure is due to the sorting phenomenon.

This analysis will make use of a unique data set on 3,416 recently hired employees in approximately as many different firms. The data set is unique because it is based on interviews conducted with the new worker's employer, and therefore contains information on the firm, the job, and the employer's opinion of the employee that have not previously been included in studies of job turnover and wage growth. The sample of recently hired workers was obtained by asking a stratified random sample of employers to provide information on an unskilled or semiskilled new hire who was hired between January 1, 1978 and October 1, 1979. (The employer survey and its sampling frame are described in Appendix A of chapter 1.) An unskilled or semiskilled worker was defined as a sales worker in the retail or service industries or a laborer, service worker, operative, or clerical worker in any industry. The employer was asked to select the most recently hired employee fitting this description regardless of whether the individual was still with the firm. A series of thirty-five questions was asked about this new employee. If the firm had hired a worker for whom it received a subsidy from TJTC, WIN, or CETA-OJT in the last two years, it was asked to answer a parallel set of questions about this worker. The sample we analyzed includes 391 workers subsidized by CETA-OJT, 44 subsidized by TJTC, and 43 subsidized by WIN.

One of the more unique elements of this data set is its measurement of the employer's opinion of the productivity of a recently hired new employee. A productivity rating was assigned to each employee at two different points in time by asking the employer or supervisor the following question, "If you consider the productivity of an average-experienced worker in this job to be 50 on a scale from 1 to 100, what rating would you give NAME for (his/her) productivity during (his/her) second week of employment?"

Comparable productivity ratings for a period of six to twenty-four months later were obtained for workers that remained with the firm by asking an identical question about current performance. A similar question was asked about the productivity of the separating worker just prior to separation. Note that all of these questions ask for a comparison between a particular worker and an average-experienced worker. They are not attempting to measure productivity in any absolute sense. They provide an ordinal indicator of the relative productivity of different workers in the same job or of the same worker at different points in time as perceived by the employer or supervisor.

8.2 Simple Tests

8.2.1 Jovanovic's Sorting Model of Wage and Productivity Growth

The longitudinal data on wage rates and an index of relative productivity make possible some simple and very direct tests of Jovanovic's sorting explanation of wage and productivity growth. Table 8.1 presents the data necessary for these tests. It includes sample means for the wage rate and the index of relative productivity separately for workers who remained at the firm, for those who quit, and for those who were terminated involuntarily.

TABLE 8.1

MEANS OF KEY VARIABLES BY TURNOVER STATUS

	All Workers	Stayers	Quitters	Layoffs and Dismissals	Standard Deviation Full Sample
Productivity Index-2nd Week	.539	.566	.513	.449	.228
Latest Productivity Index	.696	.764	.613	.500	.238
Starting Wage	$4.14	$4.11	$3.89	$4.71	
Current Wage	--	$4.56	--	--	
CETA-OJT	.122	.052	.247	.260	
TJTC or WIN	.027	.023	.036	.033	
Proportion Unionized	.107	.133	.065	.148	
Training by Managers and Peers	35	34.5	35	38	
Actual Tenure (days)	287	340	197	183	
Approximate Sample Size	3,416	2,289	718	409	

The first thing to note is that stayers have higher productivity ratings than leavers. The second week's productivity rating of those who leave voluntarily is .23 of a standard deviation lower than the rating of those who stay, and the rating of those who leave involuntarily is half a standard deviation lower. At the time they left, quitters had a productivity rating that was .63 standard deviations lower than that of stayers at the time of the employer interview. Those who separated involuntarily had a rating that was more than a standard deviation lower. This implies that sorting is indeed contributing to the observed rise of wage rates and average productivity as tenure increases.

The size of the contribution depends on the separation rate and the magnitude of the productivity differential between leavers and stayers. If we use the most recent rating of productivity as our criterion, we can see that

the attrition of about one-third of the new hires has raised the index from .696 to .764, an increase of .286 of a standard deviation.[1] If we assume that the productivity index is a proportional transformation of true productivity, we may calculate that the sorting process has increased average productivity by 10 percent. This is a large effect when one realizes it is occurring over the course of only one year and is roughly equal to the 11 percent increase in the inflation-adjusted wage that these new employees received from their employer.

8.2.2 Tests of Human Capital Theory

The second major finding one can derive from this table is the strong support it provides for human capital theory. There is evidence of considerable investment in training. In the first month of employment other workers at the firm--personnel staff, supervisors, and coworkers--spend thirty-five hours away from other activities in order to provide orientation and training to the new employee. Trainers receive higher wage rates than trainees, so the value of the time they contribute to the training process is likely to equal about one-third of the wages paid to the new employee in the first month.

There is also evidence that the investment in training pays off in higher productivity. Large improvements in productivity between the second week of employment and the interview date (or date of separation) were reported by employers. The average score on the productivity index of workers in their second week of employment is .539. Training raises this average score to .696. This is an increase of two-thirds of a standard deviation. The impact of training and experience on the index is considerably larger than the impact of sorting. About 70 percent of the increase of the productivity index from .539 to .764 is due to training and experience. The remainder is due to sorting. If we assume that the productivity index is a proportional transformation of true productivity, training is responsible for a 29 percent increase and sorting for a further 10 percent. These results imply that in unskilled and semiskilled jobs, recently hired workers are considerably less productive than workers with one or more years of tenure. In other words the Medoff and Abraham finding of a negative relationship between tenure and productivity for those with a great amount of tenure is reversed if one looks at the first year on the job.

8.3 Job Turnover Results

The next step in our analysis is the estimation of separate probit models of voluntary terminations of employment (quits) and involuntary terminations of employment (layoffs, discharges, and induced quits). These models will be used to test some of the predictions of the human capital and Jovanovic's selection theory of job turnover and wage growth, and to examine the hypothesis that there is really no true distinction between quits and dismissals.

The period for which the worker is at risk of leaving the firm varies considerably in the sample. Potential tenure, the time period between the

date of hiring and the date the employer was interviewed, has a mean of 366 days and a standard deviation of 192 days. Since the probability that an individual will have left the firm rises with the length of the observation period, potential tenure is included as a control variable in all models. We expect the rate at which new employees leave a firm to vary with tenure, so the coefficient on the potential tenure variable is allowed to shift at 160 days, 210 days, and 365 days.[2]

The nonpecuniary characteristics of the job and its skill requirements were also controlled for in all models. Measures of nonpecuniary characteristics and skill requirements of the job were available because each job had been assigned a nine-digit Dictionary of Occupational Titles (DOT) code. Information from the U.S. Department of Labor DOT job characteristics file was merged into our data set. Four of their measures of the previous training required by the job were used as control variables without modification: specialized vocational preparation (SVP), general educational development in language, general educational development in mathematics, and general educational development in reasoning. The other descriptors included in the DOT job characteristics file were combined into five factors: substantive complexity, motor skills required, physical requirements, interpersonal skills required, and working conditions. The definition of these variables is given in Appendix A. The definition, means, and standard deviations of all other variables are presented in table 8.2.

8.3.1 Are Voluntary and Involuntary Separations Distinct?

One of the issues that has been addressed by the recent literature on job turnover is whether the determinants of voluntary and involuntary turnover are truly distinct. In a recent paper, Borjas and Rosen (1980) conclude that "the results seem to indicate that at least for the young men in the National Longitudinal Survey (NLS) the distinction between the determinants of a quit and a layoff [involuntary job seperation] may not be very meaningful" (p. 171). A glance at table 8.3 quickly reveals that in our data on newly hired unskilled and semiskilled workers the determinants of voluntary turnover are completely different from the determinants of involuntary turnover. The sign of the coefficients in the model of voluntary turnover is the opposite of its sign in the involuntary turnover model for eighteen of the twenty-three variables presented in the table. Being older or being male decreases the likelihood of quitting but increases the likelihood of separating involuntarily. A higher starting wage rate lowers quit rates but increases one's chances of separating involuntarily. Greater education and greater relevant previous work experience are associated with higher quit rates but with lower layoff and dismissal rates. In a small nonunion establishment, new hires that are highly productive in their second week are much less likely to be layed off or dismissed, but just as likely to quit.

This pattern of coefficients is quite consistent with a sorting model of job turnover in which wage rates and other job rewards are tied to the job and are therefore not very responsive to the perceived competence of the individual that is hired. (For evidence that wage offers for particular jobs are

TABLE 8.2

VARIABLE DEFINITIONS AND DESCRIPTIVE STATISTICS

VARIABLE	MEAN	STANDARD DEVIATION	DESCRIPTION
Worker Characteristics			
Education	12.029	1.689	Years of schooling when hired
Experience	43.340	67.330	Months of prior relevant job experience
Age	27.497	10.161	Age when hired
Age-Squared/100	859.311	722.548	
Male	0.483	0.499	Sex dummy; 1 = male, 0 = female
Characteristics of Firm-Worker Match			
Relative Wage Ratio	0.620	0.263	Current starting wage for this job divided by average market wage in manufacturing for 1977-1979
Selection Investment	6.260	23.280	Hours spent recruiting, screening, and interviewing applicants for the job
Training by Management	20.103	25.448	Hours spent orienting and training new employee by management personnel in the first month
Training by Peers	14.426	25.089	Hours spent orienting and training new employee by nonmanagement personnel in the first month
Productivity 2nd Week	0.564	0.221	Productivity level of employee at second week of employment; ranges from 0 to 1
Productivity 2nd Week Interacted with Log of Estab. Size	1.734	1.071	Productivity at 2nd week multiplied by log of of firm size, December 1979
Productivity 2nd Week x Proportion Unionized	0.057	0.162	Productivity at 2nd week multiplied by proportion of workers unionized
Change in Productivity	0.202	0.165	Difference between current and initial productivity
White-Collar Job	0.478	0.499	Equals 1 if white-collar job (as defined by census code); 0 otherwise
Subsidy Programs			
TJTC	0.014	0.120	Equals 1 if employee is eligible for TJTC: 0 otherwise
WIN	0.011	0.107	Equals 1 if employee is eligible for WIN: 0 otherwise
CETA/OJT	0.063	0.244	Equals 1 if employee is eligible for CETA/OJT; 0 otherwise
Employer Characteristics			
Log Establishment Size	3.134	1.528	Log of the number of employees at establishment in December 1979
Log Establishment Size above 50	0.323	0.680	Log establishment size minus log 50 if employment is GT 50; 0 if employment LT 50
Proportion Unionized	0.111	0.289	Proportion of employees unionized
Proportion White-Collar	0.464	0.352	Proportion of employees in white-collar jobs
Proportion Craft	0.160	0.241	Proportion of employees in craft jobs
Market Characteristics			
Log Market Wage	1.889	0.133	Log of average market wage in manufacturing, from 1977-1979
Change Market Employment	0.083	0.040	Change in local labor market employment, 1977-1979
Log Market Size	11.303	1.330	Log of employment in local labor market

TABLE 8.3

PROBIT MODELS OF EARLY JOB TURNOVER

	Voluntary		Involuntary	
Worker Characteristics				
Years of Education	.028	(1.75)	- .047	(2.62)
Log Experience	.009	(.92)	- .017	(.85)
Age	-.025	(1.57)	.012	(.64)
Age Squared ÷ 100	.018	(.78)	- .020	(.72)
Male	-.068	(1.04)	.225	(2.87)
Characteristics of Firm-Worker Match				
Relative Wage Ratio	-.113	(.84)	.494	(3.60)
Selection Investment	-.0036	(1.46)	.0018	(1.60)
Training by Management	-.0019	(1.82)	.0007	(.64)
Training by Peers	-.0026	(2.26)	.0003	(.21)
Productivity 2nd Week	-.030	(0.16)	- .821	(3.91)
Log Emp. x Log Productivity 2nd Week	-.026	(1.19)	- .018	(.76)
Productivity 2nd Week x Union	-.683	(1.41)	1.190	(2.64)
White-Collar Job	-.097	(1.15)	.138	(1.37)
Employer Characteristics				
Log Establishment Size	-.025	(.28)	- .126	(1.35)
Log Establishment Size Above 50	-.041	(.57)	.195	(2.58)
Proportion Unionized	.106	(.39)	.458	(1.71)
Proportion White Collar	-.143	(1.49)	- .046	(.39)
Proportion Craft	-.279	(2.35)	.087	(66)
TJTC	-.258	(.99)	.027	(.10)
WIN	.311	(1.43)	.235	(.93)
CETA-OJT	.041	(.10)	.585	(1.37)
CETA above 210 days	.652		.200	
Market Characteristics				
Log Market Size	-.075	(.36)	.620	(2.45)
Change Market Employment	.500	(.79)	- .467	(.61)
Dummies for Industry	X		X	
Job Requirements	X		X	
Tenure Allowed to Shift	X		X	
2 x Log Likelihood	265		273	
Proportion Leaving	.21		.12	
Multiplier for Probability	.286		.20	

seldom adjusted to the individual's perceived quality, see Barron and Dunkelberg 1982). In sorting models, the productivity of workers in the job is not discovered until after they have been at the firm awhile. If productivity is below some criterion level, the worker is fired. Productivity in the second week is a proxy for realized productivity--i.e., low training costs and high future productivity. Sorting theory's prediction is supported by the large statistically significant negative impact of initial productivity upon the probability of being layed off or fired. When the interaction effects are evaluated at their means, the coefficient on the productivity proxy is approximately -.70. The elasticity of the involuntary termination rate with respect to the productivity proxy is -.63. If the most recent productivity index or an instrument for it had been substituted, the elasticity would have been even larger. In many firms the individual's compensation is not strongly related to his or her productivity at the firm (see chapter 9), so one would anticipate that the effect of productivity on quitting would be weaker than its effect on involuntary terminations. When interaction effects are evaluated at their means, the coefficient on the productivity proxy is approximately -.24 in the quit equation. The elasticity of quits with respect to the second week productivity proxy is -.18.

When models are estimated predicting current productivity or productivity in the second week, the personal characteristics that are most important are education and previous relevant work experience. Age and age squared are reduced to insignificance when they must compete with previous work experience.

We anticipate that if wage rates and quality of the job are held constant, new hires with greater education and previous work experience have better prospects of finding a good job elsewhere and are therefore more likely to quit. The coefficient on education in the quit model supports this hypothesis. It is positive and significant at the .05 level in a one tail test. The coefficient on previous work experience has the right sign but is not statistically different from zero. Education has a strong negative effect on involuntary separations because education is a good predictor of productivity growth. Workers' future contributions depend as much on their improvement as it does on their competence at the start.

The more attractive a job is, the less likely people are to leave it voluntarily. The starting wage has a negative coefficient in the quit model, as hypothesized. The relationship is not statistically significant, however. Theory suggests that a firm paying higher than average wage rates for a specific job, may demand higher-than-average productivity of its workers, and therefore, may set higher standards for hiring and retention decisions. It also suggests that firms with a reputation for laying off or firing a significant proportion of their new hires will find they must pay more to attract quality job applicants. For both these reasons we anticipate that high wage firms are more likely to lay off or fire a new employee. The coefficient is positive as hypothesized and statistically significant. The elasticity of the layoff and dismissal rate with respect to the relative wage is .5.

8.3.2 Tests of Predictions of Human Capital Theory

The primary prediction of human capital theory about job turnover is that workers who have a great deal of specific training should have lower rates of turnover. This proposition applies to workers who have completed their training or whose training is well underway. If the employer has paid for most of the costs of specific training, a significant loss is suffered if a separation occurs, so we would expect the separations over which the employer has control (involuntary separations) to be negatively related to the amount of specific training. If the employee has paid for the specific training, one would expect voluntary separations but not involuntary separations to be negatively related to the amount of specific training provided.

Our data set has direct measures of certain training inputs--the time supervisors and personnel staff spend orienting and training the new worker during the first month, and the time coworkers spend orienting and training the new worker during the first month. It provides no information on whether the training is general or specific. Consequently, the measures that are available are very imperfect proxies for the amount of specific training, the variable that is hypothesized to influence turnover. Nevertheless, the proxies have the predicted negative effect on quit rates, and the coefficients are statistically significant. The elasticity of the quit rate is -.055 with respect to supervisors' training time and -.052 for coworkers' training time.

Surprisingly, there is no impact of training time on involuntary separations. If employers do not pay any of the costs of specific training we would predict this result, but the tendency of productivity to rise more rapidly than the wage rate (see section 8.7) necessarily implies that employers are paying a major share of training costs. The explanation for this result seems to be that our proxy for training investment is also an indicator of the individual's efficiency at learning the ropes. A high value for the training time variable may imply either that training for this job is customarily time consuming or that this particular employee is a slow learner. If it is the latter, the variable is a negative proxy for quality and one would expect it to be positively associated with layoff and discharge rates. No doubt it is a mixture of both, hence, the small and insignificant coefficient. Note that the impact of worker quality on quits is either positive or less negative than it is for layoffs and dismissals. Consequently, the coefficient on training time is not likely to be as biased in the quit equation as it is in the layoff and dismissal equation.

The time spent selecting the new hire is an investment in specific human capital. If these costs are high, there is an incentive to keep turnover down. One criterion that firms use to select new hires is the perceived probability of a quit or a dismissal. One would hope that investing more time in the selection process would lower the realized quit and dismissal rates. There is weak evidence (a negative coefficient with a t statistic of 1.46) of such an impact on the quit rate. [Meitzen obtained a significant negative coefficient on selection investment in his model of quit rates.] The coefficient on selection investment in the layoff and dismissal model is positive and almost significant. An after-the-fact explanation for this anomolous result

is that firms that set high hiring standards also set high standards for retention, so we would expect a positive correlation between rejection rates in the selection amongst job applicants and rejection rates of new employees during their tryout period.[3]

8.3.3 Impact of Unionization and Establishment Size

A number of studies have found that representation by a union has significant impacts on job turnover. It has been found to reduce quits and discharges and to increase layoffs. Our results are consistent with these previous findings. When interaction terms are evaluated at their mean, the coefficient on the proportion unionized is -.263 in the quit equation and +.185 in the involuntary separation equation. This implies that working at a firm that is a 100 percent unionized lowers the quit rate by approximately 36 percent and raises the involuntary separation rate by about 31 percent. The coefficients on unionization and its interactions in the quit equation are all statistically insignificant, however.

The most interesting finding is the large statistically significant coefficient on the interaction between productivity (second week) and unionization in the model of involuntary separations. This interaction term implies that in a unionized firm there is no tendency for less productive employees to have a higher probability of being terminated involuntarily. These results suggest that even during a union worker's probationary period, involuntary terminations are generally layoffs based on seniority and seldom dismissals based on poor performance. The coefficients on both the productivity-unionization interaction and the productivity-establishment size interaction are negative and insignificant in the quit equation.

We saw in section 8.2 that the correlation between tenure and productivity is partly due to the tendency for the most productive workers to be retained and the least productive workers to be released. In table 8.4 we present evidence that the selection/sorting process affects the tenure and productivity relationship differently in union and nonunion firms.

The impact of the sorting process on average productivity can be approximated by calculating the ratio of the current productivity index of stayers to the index of the current or most recent productivity of all new hires. For nonunion firms this ratio is 141/127 or 1.11, implying that the productivity of the typical experienced worker is 11 percent higher than it would have been if the probability of separation had not been negatively correlated with productivity. For unionized firms this ratio is 1.06, implying that the selection process raises the productivity of a unionized work force by only half as much--about 6 percent. The smaller impact of sorting on union firms is a consequence of:

a. the lower quit rates of union firms;

b. the lower dismissal rates of union firms (note that the workers who are dismissed are considerably less productive than workers who remain or leave for other reasons);

c. the lack of any tendency to lay off the least productive worker at union firms. At nonunion firms, in contrast, workers who are laid off are considerably less productive than workers who are retained.

TABLE 8.4

PRODUCTIVITY INDEX NUMBERS
BY UNIONIZATION AND TURNOVER STATUS

	All	Stayers	Quits	Fires	Layoffs
Less than 10 Percent Union					
Percent in Column	100	65	23	7	5
Productivity 2nd Week Index	100	106	95	69	93
Latest Productivity Index	127	141	111	64	106
More than 70 Percent Union					
Percent in Column	100	70	12	4	14
Productivity 2nd Week Index	100	102	87	63	112
Latest Productivity Index	128	136	107	62	124

One consequence of the unionized firm's inability to select out the poor performers after they are hired is greater efforts to select high quality employees in the hiring decision. The staff time devoted to selecting the right person for a job opening is 2.25 hours per interviewee in a unionized establishment and 1.25 hours per interviewee in a nonunion establishment. The total time devoted to the task of selecting a new unskilled employee is nine hours in the unionized establishment and 5.7 hours in the nonunion establishment.

The main effects of establishment size imply that rates of voluntary and involuntary turnover are smaller at large establishments. A new unskilled employee at a one-employee firm is 80 percent more likely to quit and 180 percent more likely to be laid off or dismissed than a new employee at an establishment with 50 employees. A new employee at an establishment with 500 employees is about 45 percent less likely to quit than and as likely to be layed off or dismissed as a new employee at an establishment with 50 employees.

8.3.4 Impact of Subsidy Programs

None of the coefficients on the dummy variables representing subsidization by a targeted employment and training subsidy program are statistically significant. Point estimates of their impact, however, suggest that workers

subsidized by WIN or CETA-OJT are considerably more likely to quit or be terminated involuntarily. Being WIN eligible is associated with a 40 percent higher probability of quitting or being fired or laid off. Because CETA-OJT subsidies last for only six months, it was hypothesized that for workers hired under this subsidy, there would be a considerably higher probability of a separation having occurred if the employer interview is conducted at least six months after the date the employee was hired. Special tenure variables were defined for CETA-OJT workers to capture this effect. The calculated impact of CETA-OJT at potential tenure of seven months or more is presented in table 8.3 below the CETA-OJT coefficient. These coefficients imply that if the individuals were hired at least seven months previously, the CETA-OJT trainee has a quit probability of about 44 percent rather than the 21 percent quit probability of the typical unsubsidized worker. Surprisingly, rates of involuntary turnover are much less affected by being a CETA-OJT trainee. CETA-OJT trainees who were hired at least seven months previously had a 16 percent probability of being laid off, rather than the 12 percent probability of the typical unsubsidized worker. It would seem that most of the attrition of disadvantaged workers from their subsidized jobs is voluntary rather than involuntary. We find no evidence that once the subsidy period is over, employers are laying off, firing, or forcing their subsidized workers to quit. It may be that some CETA prime sponsors seek to place CETA-OJT trainees at other firms once the six-month training period is over and that the subsidized employer perceives these outcomes as quits rather than layoffs. More study of the implicit and explicit understanding that goes with a CETA-OJT contract is required.

8.4 The Determinants of Employer Training Investments

Learning how to do a job is a process that takes time and effort. The most important input into the learning process is the time and effort of the trainee. In most cases, however, the trainees cannot learn all that they need to know entirely on their own. Complementary inputs of time are required from others: supervisors, coworkers, and sometimes specialized training personnel. The employer survey asked two questions about these complementary inputs: (a) "In the first month of _____'s employment approximately how many hours did personnel and supervisory staff spend orienting and training the new employee" and (b) "In the first month approximately how many hours did employees other than personnel and supervisory staff spend away from their normal work routines orienting and training the new employee?"

The answers to these questions are examined in this section. Models predicting the amount of time personnel and supervisory staff spent training the new hire and the amount of time other coworkers spent training the new hire were estimated by ordinary least squares. The results are presented in table 8.5.

8.4.1 The Impact of Worker Characteristics

One would expect that new workers whose previous work experience is relevant to their new jobs would require less training. This hypothesis is

TABLE 8.5

THE TIME SPENT TRAINING A NEW EMPLOYEE

	Training by Management		Training by Coworkers	
Worker Characteristics				
Education LT 4	- 9.65	(.36)	-19.12	(.79)
Yrs of Education	- 2.70	(.80)	- 4.10	(1.35)
Yrs of Education GT 8	3.86	(1.03)	5.20	(1.53)
Yrs of Education GT 12	.28	(.36)	- .83	(1.15)
Experience ÷ 100	- 3.02	(1.81)	- 5.00	(3.31)
Experience Squared ÷ 10,000	.17	(.33)	.87	(1.89)
Age	- .26	(.96)	- .08	(.31)
Age Squared	.0034	(.85)	.0004	(.10)
Male	2.27	(2.12)	0.20	(.20)
Subsidy Programs				
TJTC	- 3.97	(1.03)	.66	(.19)
WIN	- 4.21	(1.14)	3.86	(1.16)
CETA-OJT	9.28	(6.77)	5.94	(4.78)
Employer Characteristics				
Log Establishment Size	- 2.57	(5.71)	1.62	(3.98)
Log Establishment Size GT 50	5.24	(4.89)	- .11	(.11)
Proportion Unionized	.30	(.17)	- .87	(.54)
Proportion White Collar	.21	(.14)	5.85	(4.34)
Proportion Craft	4.65	(2.34)	4.61	(2.57)
Market Characteristics				
Log Market Wage	.22	(.07)	- .39	(.13)
Dummies for Industry	X		X	
Job Requirements	X		X	
Dummies for Wage Rate	X		X	
R Square	.064		.049	
Mean of Dependent Variable	20.8		14.4	

supported by the negative and significant coefficients on experience in our model. Two years of such experience reduces management training time by .71 hours (a reduction of 3.4 percent) and coworker training time by 1.15 hours (a reduction of 8 percent).

Theory yields only a weak prediction about the impact of education on the time others will spend training the worker. Educated workers are presumed to be more efficient learners. They should be able to learn a given set of skills more quickly. Holding the final competence of the worker constant, an educated worker should require less training time than one with less education. This would imply a negative relationship between education and training inputs. On the other hand, it will generally be profitable to teach more and higher level skills to the quickest and most efficient learners. This generates the opposite prediction. More will be taught to the educated, implying a positive relationship between education and training inputs. An examination of the coefficients on the education variables in table 8.5 suggests that the net effect of education on training time is positive. When all four variables are entered simultaneously, none of the coefficients are statistically significant, however. When a single year of education variable is substituted for four different variables, it is almost significant ($t = 1.56$) in the coworker training time equation and highly significant ($t = 4.2$) in the management training time equation. These results imply that even when job characteristics are controlled, the tendency of more educated workers to be taught more and higher level skills outweighs the educated worker's greater speed at learning particular skills.

Males typically receive 2.27 hours more training time from management. Since males usually show smaller gains in productivity than females, the greater number of training hours implies either that males are slower learners or that the types of jobs they typically fill require a greater amount of managerial instruction and supervision in the first month than the jobs typically filled by women.

8.4.2 The Impacts of Employer Characteristics

The size of the establishment has an important effect on the pattern of training investments in a worker. In very small establishments management tends to provide the bulk of the training. We estimate that in a typical two-employee firm the manager spends about twenty-six hours on training and coworkers spend about eleven hours. In medium-sized establishments (those with about 50 employees) training responsibilities are shared more equally. Supervisors provide an average of about eighteen hours of training, and coworkers provide about sixteen hours. Training time investments are most extensive in large establishments. At establishments with 500 employees we estimate that management provides an average of twenty-five hours of training, and coworkers provide about nineteen hours.

Unionization has no effect on the amount or distribution of training time investments. Establishments with high proportions of their workers in craft occupations provide more of both kinds of training than establishments with no

craftworkers. Establishments with high proportions of their workers in white-collar occupations offer higher-than-average amounts of training by coworkers and roughly equal amounts of training by managers.

8.4.3 The Impact of Subsidy Programs

The CETA-OJT program is supposed to be a subsidy of training. The coefficients on the dummy for CETA-OJT imply that new employees who are subsidized by this program do indeed receive more training than unsubsidized workers: about nine extra hours from management and six extra hours from coworkers. Thus the program does seem to increase the time the firm invested in training new workers. In the first month, CETA-OJT subsidized workers receive a total of approximately fifty hours of training from supervisors and coworkers. The sample of participants in TJTC and WIN is small so none of their coefficients are statistically significant.

8.5 Productivity Growth Results

In this section we analyze the determinants of productivity growth. Our dependent variable is the change in the index of relative productivity multiplied by 100. The mean of the variable is 15.77 points for all workers and 19.86 for those who remain with the firm. The mean of the index of relative productivity for those still at the firm at the time of the interview is 76.4 points.

Table 8.6 presents the results of estimating our model on two different samples. The first two models present the results when the full sample is used. The full sample includes workers who left the firm before the survey interview date, as well as workers who were still at the firm on the interview date. The longer a worker is at the firm, the more effective that individual is likely to become so the actual tenure at the time of the second measurement of productivity is included as a control variable. Since, however, slow learners are more likely to leave the firm and therefore have short tenure, coefficients on the tenure variables may be biased. One way to avoid this bias is to limit the sample to workers who have not left the firm. Models of this type are subject to another type of bias: selection bias. Mechanisms are available for correcting this bias, however. Model 3 corrects for selection bias by entering the Mills ratio derived from a probit model of job retention. The rationale for this procedure is developed at greater length in chapter 9.

8.5.1 The Time Pattern of Productivity Growth

Medoff and Abraham (1981a, 1981b) have presented persuasive evidence that in large firms the most senior employees are often somewhat less productive than the less senior employees found in the same position. Evidence was presented in section 8.2 that in our sample, productivity ratings rise over the course of the first year of a new hire's employment. There is no

TABLE 8.6

THE DETERMINANTS OF CHANGE
IN THE PRODUCTIVITY INDEX

	Full Sample				Stayers	
	Model 1		Model 2		Model 3	
Worker Characteristics						
Education LT 4	2.25	(.12)	-11.82	(.70)	-18.58	(.97)
Yrs of Education	- .79	(.35)	- 2.10	(1.00)	- 2.94	(1.24)
Yrs of Education GT 8	1.54	(.61)	3.39	(1.42)	4.12	(1.55)
Yrs of Education GT 12	- .37	(.68)	- 1.01	(2.00)	.89	(1.65)
Experience ÷ 100	-2.89	(2.54)	.42	(.40)	1.72	(1.47)
Experience Squared ÷ 10,000	.72	(2.06)	.13	(.42)	.28	(.77)
Age	- .31	(1.67)	- .15	(.86)	- .04	(.23)
Age Squared	.003	(1.27)	.002	(.68)	.001	(.31)
Male	-1.29	(1.96)	- .77	(1.12)	- 1.32	(1.75)
Characteristics of Firm-Worker Match						
Selection Investment	--	--	.000	(.05)	.003	(.20)
Training by Management	--	--	.050	(4.66)	.087	(7.21)
Training by Peers	--	--	.025	(2.12)	.039	(3.00)
Productivity 2nd Week	--	--	- .32	(25.37)	- .33	(17.94)
Subsidy Programs						
TJTC	1.51	(.60)	- .43	(.20)	- .18	(.08)
WIN	-1.23	(.49)	- 3.16	(1.37)	- 3.91	(1.40)
CETA-OJT	1.48	(1.59)	- 2.10	(2.36)	- 6.96	(3.10)
Employer Characteristics						
Log Establishment Size	.87	(2.86)	.85	(2.95)	2.30	(3.78)
Log Establishment Size GT 50	.05	(.06)	- .70	(1.01)	- 1.77	(2.22)
Proportion Unionized	.75	(.63)	.97	(.86)	.24	(.21)
Proportion White Collar	4.41	(4.52)	1.43	(1.51)	.38	(.96)
Proportion Craft	2.67	(2.04)	.38	(.31)	1.38	(.96)
Market Characteristics						
Log Market Wage	-1.05	(.47)	- 2.66	(1.23)	- 2.26	(.96)
Change Market Employment	--	--	- 2.21	(.32)	5.39	(.74)
Tenure Variables						
Actual Tenure	.003	(.14)	.035	(1.63)	.041	(.70)
Tenure GT 3 months	.105	(3.23)	.068	(2.23)	- .097	(1.30)
Tenure GT 6 months	- .075	(4.48)	- .079	(5.02)	.059	(2.18)
Tenure GT 12 months	- .016	(2.12)	- .012	(1.70)	.005	(.65)
Lambda	--	--	--	--	17.01	(3.78)
Dummies for Industry	X		X		X	
Job Requirements			X		X	
Dummies for Wage Rate	X		X		X	
R Square	.120		.276		.350	

contradiction between these two findings for they refer to quite different segments of the tenure-productivity relationship.

Is there evidence in our data of a slowdown in the rate of productivity growth as tenure increases? Yes, there is strong evidence of such a slowdown. Because it controls for inputs of training time and productivity in the second week, model 2 provides the best measure of the time pattern of productivity growth. The coefficients on the tenure splines imply that the productivity index rises at a rate of 1.05 points per month in the first three months of employment, at a rate of 3.09 points per month in the next three months, at a rate of .72 points per month in the next six months, and at a rate of .36 points per month thereafter. If we assume that the productivity index is a proportional transformation of true productivity, percentage rates of increase may be calculated. They are 1.9 percent per month in the first three months, 5.4 percent per month in the next three months, 1.1 percent per month in the next six months, and .55 percent per month thereafter. These rates of increase are quite substantial. Even the rate of increase for the period after one year is impressive: 6.6 percent per year.

As previously mentioned the coefficients on the tenure variables in model 2 may be biased. Model 3 is not subject to this bias. The number of observations having fewer than six months of tenure is too small to produce stable estimates of tenure slopes during the first six months of employment, however, so we will focus on the period after six months of employment. For the period beyond six months, model 3 is a potentially useful check on our model 2 findings. The slope is .12 percent per month for month six through twelve and .30 percent per month (3.5 percent per year) thereafter. It would seem that the point at which productivity starts declining with tenure has not been reached in our data (166 of the 2,248 observations in the stayer sample have two or more years of tenure).[4]

8.5.2 The Payoff to On-the-Job Training Investments

In this section we attempt to measure the payoff for time spent by employees (supervisors, personnel office staff, and coworkers) in training the new employee. Measures are available of (a) the time personnel and supervisory staff spent orienting and training the new employee and (b) the time employees other than personnel and supervisory staff spent away from their normal work routines orienting and training the new employee. Both of these indicators of the employer's efforts to train the employee had large statistically significant positive impacts on the change in the index of relative productivity.

The firm can be assumed to be attempting to minimize the costs of training the new employee. This implies the familiar marginal condition that the ratio of these marginal costs of each training input should equal the ratio of the marginal productivity of each type of training input. The marginal condition for training inputs is

$$(1)\quad \frac{\dfrac{d(P_1-P_0)}{dT_m}}{\dfrac{d(P_1-P_0)}{dT_c}} = \frac{P_m + P_0}{P_c + P_0}$$

This formulation assumes that training is one on one and an hour of trainee time is associated with an hour of trainer time. The marginal condition that defines the relative size of P_m and P_c is

$$(2)\quad \frac{P_m}{P_c} = \frac{W_m}{W_c}$$

where P_0, P_1 = Trainee productivity before and after training, respectively

P_m, W_m = Marginal productivity and wage rate of management staff who provide training

P_c, W_c = Marginal productivity and wage rate of coworkers who provide training

T_m, T_c = Time spent training the new employee by management and coworkers, respectively.

Because they receive higher wages, an hour of training provided by personnel or supervisory staff is more costly to the firm than an hour of training provided by a coworker. In other words, since $W_m > W_c$ implies $P_m > P_c$, P_m+P_0 must be greater than P_c+P_0 and $d(P_1-P_0)/dT_m$ must be greater than $d(P_1-P_0)/dT_c$. The coefficients we obtain on our measures of training by management and training by peers are estimates of $d(P_1-P_0)/dT_m$ and $d(P_1-P_0)/dT_c$, respectively. As predicted, the coefficients on training by management are larger (more than twice as large) than the coefficients on training by peers, and the difference is statistically significant.

Calculating a rate of return to this investment requires that we make a number of assumptions about the scaling of the index of relative productivity and about the relative size of P_m, P_c, and P_0. Our assumptions are that when comparing a worker's productivity at different points in time at the same firm, the productivity index is a proportional transformation of true productivity and a multiplicative error.

$$(3)\quad P_{ij} = a_j P^T_{ij} e^{u_{ij}}$$

where P_{ij} = the productivity index for the i^{th} worker at the j^{th} firm

a_j = an unknown constant unique to the firm

P^T_{ij} = true marginal product of the i^{th} worker at the j^{th} firm.

The yearly real rate of return may be defined as

$$(4)\quad \text{Real Rate of Return} = \frac{\left(\begin{matrix}\text{Probability}\\ \text{of Staying}\end{matrix}\right) \cdot \left(\begin{matrix}\text{Benefit}\\ \text{at Firm}\end{matrix}\right) + \left(\begin{matrix}\text{Probability}\\ \text{of Leaving}\end{matrix}\right) \cdot \left(\begin{matrix}\text{Benefit at}\\ \text{Other Firm}\end{matrix}\right)}{\text{Cost of Trainer Time + Cost of Trainee Time}}$$

The mean productivity of a new hire is 53.9, and 67 percent of the new hires stay. We will assume that the productivity index for coworkers (P_C) is equal to the productivity of the trained worker $P_C = P_1 = 76.4$ (see table 8.1). The comparable index number for personnel and supervisory staff is assumed to be twice P_C or 162.8. Using model 3's estimates of $d(P_1-P_0)/dT_m$ and $d(P_1-P_0)/dT_C$ we have

$$RR_{Management} = \frac{.67(2{,}000 \text{ hrs } .087) + .33\ (2{,}000 \text{ hrs} \cdot b)}{162.8 + 53.9} = 54\% \text{ for } b=0$$

$$RR_{peers} = \frac{.67(2000 \text{ hrs } .039) + .33\ (2{,}000 \text{ hrs} \cdot b)}{76.4 + 53.9} = 40\% \text{ for } b=0$$

While these estimates of the rate of return to inputs of trainer time depend upon a number of assumptions that may be challenged, they are, nevertheless, quite interesting.[5] These rates of return are quite high. They imply that small increases in the time spent training a new employee have a payback period of 2 to 2.5 years. High initial rates of return are to be expected for this kind of training investment. Some of the skills being taught become obsolescent with time (because the worker is transferred to another job in the firm or the job is restructured because of technological change) or lose their usefulness when the worker leaves the firm.

8.3.3 The Impact of Worker, Employer, and Market Characteristics

Models 2 and 3 are structural models of the impacts of worker and employer characteristics that control for the worker's productivity in the second work week and the employer's training investments. Model 1 is a reduced form that does not control for job requirements, initial productivity, and the employer's training investments. The coefficients on worker and employer attributes in this model capture both the direct effects and the indirect effects that operate through initial productivity and employer investment decisions.

In the structural models, experience and education have small, positive effects on productivity growth that are generally not significant. The impact of age is negative, small, and statistically insignificant. The size of the establishment has a large, statistically significant impact upon productivity growth. The change of the productivity index for a new employee at a fifty-employee establishment is 7.4 points (2.7 points in model 2) greater than it is at a two-employee establishment. None of the other characteristics of the employer or the market have a large or statistically significant impact on the change of the productivity index.

The reduced form model yields somewhat more interesting results. New hires with relevant previous experience generally have less to learn (have higher initial productivity) and require less employer investment. As a result the coefficient on experience that is positive in the structural models becomes negative and statistically significant in the reduced form model.

The reduced form model also implies that new hires at firms with high proportions of white-collar jobs and high proportions of craft jobs experience higher rates of productivity growth.

None of the characteristics of the local labor market have a statistically significant effect on the change in our index of productivity. In models not reported here, however, we do find large, statistically significant negative effects of wage rate and the growth of local employment on current productivity of workers with six or more months of experience. This finding provides support for the hypothesis that when labor markets are tight, firms adjust both by raising their wage and lowering their hiring standards. Making the standard set of assumptions about the scaling of the productivity index, the coefficients on these variables imply that 10 percent higher local manufacturing wage rate is associated with a 2 percent reduction in the average quality of new hires. A two standard deviation increase in the growth rate of local employment (4.15 percent per year) is associated with 2.4 percent reduction in average quality of new hires.

8.5.4 Impact of Subsidy Programs

The coefficients on the subsidy program dummies in the structural models are all negative. Two of them are of reasonable size and the CETA-OJT coefficient is significantly less than zero. These results imply that recipients of WIN and CETA-OJT subsidies are slower learners than the typical, unsubsidized employee. Given the targeted nature of the programs, this should not come as a surprise. The purpose of the program is to induce firms to hire workers who lack the experience and trainability that the firm normally demands of new employees.

Despite their lower trainability, new hires who bring a subsidy to their employer typically seem to improve their productivity just as much as unsubsidized employees. This is the implication of the small, statistically insignificant coefficients on the program dummies in the reduced form model. Their productivity improves a roughly equal amount because they start from a lower base and employers typically invest more time in training them.

8.6 Does the Minimum Wage Discourage On-the-Job Training

A number of economists have charged that the minimum wage discourages on-the-job training of inexperienced and unskilled workers (Hashimoto 1981b, Leighton and Mincer 1981). Let us examine the reasons for expecting the minimum wage to have this impact. Providing training to a new employee is costly. The new employee is not very productive at first, and other workers must take time away from their regular activities to give instruction to the new hire. Many of the skills that the new employee learns have application in other firms as well. To avoid losing the worker to another firm, the employer that is providing the training must raise the wage as the trainee's productivity increases. Jobs that offer training and the prospect of future wage increases

are more attractive than those that do not. The competition for these jobs will enable employers that offer on-the-job training in generally useful skills to obtain workers at low wage rates. Minimum wage legislation, however, prevents wage rates from falling below the legislated minimum. Lacking the ability to get new employees to pay a major share of the costs of general training (by accepting a low wage during the training period), employers will adopt production technologies that minimize the skill requirements of the job. The evolution of the diner and the small, family-operated restaurant into franchised fast food operations using specially designed machines and already prepared food is an example of how this is accomplished. By reducing the skills required to do the job, the employer shortens the time it takes for new employees to reach maximum productivity. The same person may have the job but is taught less, and what is taught is useful only in that firm--not elsewhere. Opportunities for promotion are minimal, and wage increases are small or nonexistent.

While the theoretical case for the proposition that the minimum wage discourages OJT is strong, very little empirical evidence of such an effect has been presented. Direct measures of OJT have not been available. Efforts to test this hypothesis have had to use indirect methods that have not yielded conclusive results (Hashimoto 1981b). The employer data set contains two measures of inputs into on-the-job training--time spent training by management and time spent training by coworkers--and one measure of training output--the change in rated productivity of the worker. These measures make possible a more direct test of the impact of the minimum wage on OJT.

In the absence of a minimum wage, we would expect that for people with a given set of credentials, there would be a negative correlation between the training provided on a job and its wage rate. We hypothesize, therefore, that wage rate and OJT are negatively correlated in ranges of the wage distribution that are unconstrained by the minimum wage. This hypothesis is tested by entering a continuous measure of the starting wage rate. A negative coefficient is expected on this variable.

If the minimum wage is having an effect on investment in OJT, its effect will be visible in the jobs whose starting wage is at or below the minimum wage. Many of these jobs will have had to be redesigned to minimize training time and the development of general skills. At the time of our interviews, the spring, summer, and fall of 1980, the minimum wage was $3.10 an hour. The new hire about whom the wage rate and productivity questions were asked was hired in either 1979 or 1978 when the minimum wage was $2.90 and $2.65 respectively.[6] Dummies were defined for wage rates less than $2.75, wage rates between $2.75 and $3.05, wage rates between $3.05 and $3.15, and wage rates between $3.15 and $3.50. We hypothesize that the dummies for the three lowest wage rate categories will have negative coefficients. If our hypotheses are confirmed there will be a curvilinear relationship where, holding job requirements and worker credentials constant, jobs offering the least amount of training are those paying at or below the minimum wage and those paying very high wage rates.

The empirical results are presented in table 8.7. As hypothesized, the continuous measure of the wage rate has a negative coefficient in all three

equations, two of which are statistically significant at the .05 level on a one tail test. All the coefficients on the dummies capturing the effect of the minimum wage are negative as hypothesized. Seven of the nine coefficients are statistically significant at the .025 level on a one tail test. They imply that jobs paying $3.10 an hour offer 3.3 fewer hours of training by management (a reduction of about 15 percent) and 4.5 fewer hours of training by coworkers (a reduction of abut 30 percent). The growth of the productivity index is 2 points lower (a reduction of about 15 percent).

TABLE 8.7

IMPACT OF THE MINIMUM WAGE ON
ON-THE-JOB TRAINING

	Training by Management	Training by Peers	Change of Productivity Index
Wage LT $2.75	-8.44 (2.26)	-5.44 (2.65)	-4.14 (2.70)
Wage $2.75 - $3.05	-2.99 (2.19)	-2.66 (1.34)	-2.89 (1.96)
Wage $3.05 - $3.15	-3.33 (1.34)	-4.52 (3.72)	-2.18 (2.43)
Wage $3.15 - $3.50	-1.43 (1.33)	- .10 (.80)	-1.47 (1.64)
Wage Rate	- .64 (1.94)	- .37 (1.24)	- .36 (1.64)

NOTE: The first two columns of results are the wage coefficients for the models presented in table 8.5. The final column of wage rate coefficients is for model 1 in table 8.6.

8.7 Comparing Wage Growth and Productivity Growth

The analysis presented in previous sections of this chapter has tended to support both the sorting and human capital theories. The data contain one very important challenge to both these theories. With such large training investments and large productivity improvements, it is hard to understand why the average wage increase is so small, only 11 percent. If we assume that the productivity index is a proportional transformation of true productivity, the coefficient of variation of true productivity is .31, and new employees are only 70 percent as productive as experienced employees. To arrive at an estimate of the productivity net of training costs of an employee in the first month on the job we must also subtract the value of the training investment by management and coworkers. The value of training investments was calculated in section 8.2 to be equal to one-third of the wages paid to the new employee in the first month. Thus, our data imply that during the first month of employment in an unskilled or semiskilled job, that productivity net of training costs is about 40 percent of the productivity of a worker with about one year of experience.[7] If the coefficient of variation of true productivity is .15

instead of .31, productivity net of training costs in the first month is still only 55 percent of later productivity. Either way, productivity net of training costs is rising at least four times faster than wage rates.

This is consistent with human capital theory only if almost all training is specific and employers pay almost all the costs of specific training. How plausible is this? Hashimoto's theory of the sharing of investments in specific human capital states that employers will pay most of the costs of such investments in these instances: when fire rates are high and quit rates are low, when the positive impact of higher wages on the rate of involuntary separations is greater than their negative impact on quit rates, and when future wage rates cannot be made contingent on performance.

In our data quits are twice as likely as layoffs and dismissals. Involuntary terminations, however, are more strongly related to the firm's wage than quits (see section 8.3), and the elasticity of one's wage with respect to one's productivity is considerably below 1 (see chapter 9). These latter two findings suggest that employers will pay most of the cost of specific training. A further reason for expecting employers to pay a large share of specific training costs is that firms have better access to capital markets and may, therefore, not demand as high a rate of return on a training investment as a worker. We conclude, therefore, that it is plausible for employers to pay most of the costs of specific training.

The proposition that all or almost all of the training must be specific is much less plausible, so the puzzle remains. The evidence we have cited suggests to this author that implicit contracts may exist whereby new workers implicitly promise not to take a new job quickly and employers offer to pay for some of the general training that the new employee needs. This might occur for a number of reasons:

a. The minimum wage might prevent the low starting wage that is necessary for the employee to pay the full costs of general training. (Evidence that the minimum wage does indeed have that effect was presented in section 8.6.)

b. The impact of starting wages on the supply and quality of labor (through effecting changes in job applications) might be much greater than the impact of wage rates received by more experienced workers on the supply and quality of labor (through its effects on both quit rates and job applications).

c. The promise not to take another job might be enforceable in part through antipirating understandings and a hiring preference for job seekers who have exhibited a tendency to stick with a job for awhile (under these conditions, a quit damages the employee's reputation).

NOTES

1. This may overstate the impact of sorting because one of the reasons leavers have lower ratings at seperation is that they have not been there as long as the typical stayer.

2. The coefficients on these variables are not reported in table 8.3 to conserve space. The probability of having left does rise with potential tenure but the rate of leaving seems to be much higher in the first 210 days than it is later.

3. If this interpretation is correct, the standards a firm sets in selecting new hires and for retaining new hires are jointly dependent and their effect on each other can be researched only by estimating a structural model. Estimation of a structural model that captures the full complexity of the endogencity of the firms selection, training, pay and turnover policies is not feasible in this data.

4. The growth rate of wage rates during these time intervals are rather similar to these calculated productivity growth rates. Wage rates grew 0.36 percent per month between the sixth and twelfth month and 0.49 percent per month thereafter. The data does not reject the hypothesis that the growth rates of wages and productivity are the same in corresponding time intervals. Some employers may have mistakenly reported the actual starting wage for that individual rather than the current starting wage for the job. If so, some observations are not adjusted for inflation of the scale wage and these measures of the growth of the real wage are upward biased.

5. Biases can arise from a number of sources: errors in measurement of training time, a correlation between training investments in the first month and later training investments, lack of a measure of the time spent in training activities by the trainee, and an incorrect assumption about the proper scaling of the productivity index.

6. The employer was asked "what is the current starting wage of this job". If the employee we were discussing had been hired in 1978 at the then minimum wage of $2.65, the employer was supposed to answer $3.10 an hour, the level of the minimum wage at the time of the interview. We suspect, however, that some respondents misinterpreted our question and reported the individual's actual starting wage rate. Thus a report that a starting wage rate of a covered job is below $3.10 does not necessarily imply the law was being violated.

7. This estimate assumes that one year after being hired that training by others has diminished to zero. If it was assumed instead that training by others has only been reduced by half, the growth of productivity net of training costs is still at least three times the growth rate of wage rates.

APPENDIX 8A

THE DOT AS A SOURCE OF OCCUPATIONAL DATA

Factor Analysis of Dictionary of Occupational Titles Occupational Characteristics, Items, and Loadings for Six Factors

	Factor 1: SUBSTANTIVE COMPLEXITY	
GED	General educational development	+
SVP	Specific vocational preparation	+
INTELL	Intelligence	+
DATA	Complexity of functioning with data	+
REPCON	Repetitive or continous processes	-
NUMBER	Numerical aptitude	+
VERBAL	Verbal aptitude	+
ABSTRACT	Abstract and creative vs. routine, concrete activities	+
MVC	Measurable or verifiable criteria	+
CLERICAL	Clerical perception	+
SPATIAL	Spatial perception	+
PEOPLE	Complexity of functioning with people	+
FORM	Form perception	+
TALK	Talking	+
DCP	Direction, control, and planning	+
VARCH	Variety and change	+
DATACOM	Communication of data vs. activities with things	+
	Factor 2: MOTOR SKILLS	
FINGDEX	Finger dexterity	+
MOTOR	Motor coordination	+
MANDEX	Manual dexterity	+
THINGS	Complexity of functioning with things	+
FORM	Form perception	+
SPATIAL	Spatial perception	+
SEE	Seeing	+
REACH	Reaching	+
STS	Set limits, tolerances, or standards	+
MACHINE	Activities involving processes, machines vs. social welfare	+
	Factor 3: PHYSICAL DEMANDS	
LOCATION	Outside working conditions	+
STOOP	Stooping, kneeling, crouching, crawling	+
EYEHAND	Eye-hand-foot coordination	+
CLIMB	Climbing, balancing	+
STRENGTH	Lifting, carrying, pulling, pushing	+
	Factor 4: MANAGEMENT	
DEPL	Dealing with people	+
DCP	Direction, control, planning	+
PEOPLE	Complexity of functioning with people	+
TALK	Talking	+
TANGIBLE	Activities resulting in tangible satisfaction vs. prestige	-
SCIENCE	Scientific, technical activities vs. business contact	-
DATACOM	Communication of data vs. activities with things	+
DATA	Complexity of functioning with data	+
	Factor 5: INTERPERSONAL SKILLS	
SJC	Sensory or judgmental criteria	+
FIF	Feelings, ideas, facts	+
INFLU	Influencing people	+
MACHINE	Activities involving processes, machines vs. social welfare	-
	Factor 6: UNDESIRABLE WORKING CONDITIONS	
HAZARDS	Hazardous conditions	+
ATMOSPHR	Fumes, odors, dust, poor ventilation	+
HEAT	Extreme heat	+

CHAPTER 9

WAGE GROWTH

John H. Bishop and Stanley Stephenson, Jr.

In this chapter we examine the determinants of the wage increases that new employees receive in the first year or so of their tenure at a firm. The theoretical perspective we bring to this task has already been described in the introduction to chapter 8, and it will not be repeated here. The data set and variable definitions are also described in chapter 8. The models of growth that we will be analyzing are estimated using data on workers who were still at the firm at the time of our employer survey. The restricted nature of the sample implies that these models may be subject to selection bias. The procedure that was adopted for correcting this potential selection bias problem is described in section 9.1. The sections that follow present the empirical findings. The impact of the measured productivity of the worker on the wage increases received is examined in section 9.2. The impact of other characteristics of the worker is discussed in section 9.3. The impacts of the employer's characteristics and the pressure of demand in the local labor market are presented in sections 9.4 and 9.5.

9.1 Correcting for Selection Bias

In estimating a wage growth equation for workers in our sample, we are faced with a complex missing data problem. Since many of the workers for whom we have data were no longer with the firm at the time of final contact (i.e., they had quit or were terminated), we do not observe wage growth for all workers. If the worker's presence/absence is not the result of a random process, the distribution of wage growth values becomes truncated (or censored). If ordinary least squares is applied to the subsample of workers still at the firm, parameter estimates will be conditional on the presence of the worker. Consequently, estimates of the effects of changes in policy-related variables on wage growth will be subject to a potentially serious selection bias.

If we had a perfect measure of the probability that a given worker was at the firm at last contact, we could use this predictor as a regressor in the wage growth equation, thus relieving the parameter estimates of the condition mentioned previously. We do not have such a measure, however, and if there are unobservable characteristics of the worker or firm that influence both the probability of still being at the firm and the wage growth of the worker, then this probability will be correlated with the error term in the wage growth equation, thus violating one of the more crucial assumptions of ordinary least squares.

To circumvent the potential selection bias problem, we adapt a strategy similar to that of Heckman (1979). Heckman shows that while using a censored

sample of workers still at the firm, the missing data problem can be reformulated as a specification error resulting from the omission of an important control variable, λ. λ is defined as

$$(1) \quad \lambda = \frac{\theta(Z)}{1-\phi(Z)}$$

where θ and ϕ are respectively the density and distribution functions for the standard normal variable Z. Z is defined as $-\beta X$ where βX is the predicted probability of having valid wage data for the worker (obtained via a first stage probit on the current employment status of the individual).

Heckman's technique has the advantage of (1) allowing unbiased estimation using only workers with accurately measured wage growth and (2) taking into account the possibility of unmeasured variables affecting both the selection equation (Is the worker still with the firm?) and the outcome equation of interest (What has the worker's wage growth been?). As long as data are available on all workers, regardless of current status, a first stage probit can furnish $-\beta X$ (and thus Z), needed to construct λ.

Care must be taken, however, if the same set of independent variables is used both in the probit and the wage growth regressions. It can be seen from equation (1) that λ is simply an algebraic transformation of the predicted probability of being at the firm, obtained in the probit procedure. This probability is in turn a function of the independent variables. Now, in theory there is no identification problem in the system, since the probit utilizes a different functional form than does the wage growth equation. In practice, however, λ often can be highly correlated with other explanatory variables in the wage growth regression, resulting in problems of collinearity, or in extreme cases, an ill-conditioned X-prime-X matrix.

9.2 Impacts of the Employee's Productivity

The most important finding of this chapter is the positive and significant impact of our index of productivity in the second week and the growth of productivity on the wage increases received by an employee (see table 9.1). Holding productivity growth constant, a one standard deviation increase in the index of productivity in the second week (an increase of .23 units) is associated with a 2.8 percent higher wage increase in firms with only one employee. Holding initial productivity constant, a similar increase in productivity growth is associated with a 4.0 percent higher wage increase in firms with only one employee. The interaction between the index of current productivity and firm size has a statistically significant negative impact on wage growth. This implies that the responsiveness of wage growth to the individual worker's relative productivity is greatest in small firms. The impact of a one standard deviation change in initial productivity on wage growth is 2.8 percent in a one-employee establishment, 1.0 percent in an establishment with 50 employees and zero in an establishment with 500 employees. The impact of a similar change in the index of productivity growth on wage growth is 4.0 percent in

TABLE 9.1

DETERMINANTS OF WAGE GROWTH

	Log Wage Growth	Log Wage Growth	Log Starting Wage
Worker Characteristics			
Education LT 4	-.48 (2.43)	-.47 (2.41)	-.36 (1.35)
Years of Education	-.054 (2.21)	-.056 (2.27)	-.038 (1.14)
Years of Education GT 8	.076 (2.81)	.80 (2.92)	.071 (1.89)
Years of Education GT 12	-.018 (3.27)	-.019 (3.39)	-.011 (1.44)
Experience ÷ 100	.024 (2.03)	.025 (2.12)	.111 (6.55)
Experience Squared ÷ 10,000	-.004 (1.19)	-.005 (1.30)	-.021 (4.27)
Age ÷ 10	.069 (3.64)	.065 (3.40)	.228 (8.21)
Age Squared ÷ 100	-.010 (3.84)	-.010 (3.66)	-.030 (7.61)
Male	.063 (8.13)	.063 (8.17)	.164 (15.31)
Characteristics of Firm Worker Match			
Selection Investment ÷ 100	-.011 (.71)		
Training by Management ÷ 100	.031 (2.49)		
Training by Peers ÷ 100	-.001 (.08)		
Productivity 2nd Week	.123 (3.19)	.056 (1.62)	
Growth in Productivity	.173 (4.53)		
Size x Latest Productivity	-.020 (1.99)		
Size x Productivity 2nd Week		-.016 (1.76)	
Log Starting Wage	-.133 (10.58)	-.130 (10.41)	
Subsidy Programs			
TJTC	.004 (.05)	-.003 (.14)	-.021 (.54)
WIN	.016 (.56)	.008 (.61)	.024 (.03)
CETA-OJT	-.010 (.41)	.006 (.25)	-.022 (1.54)
Employer Characteristics			
Log Establishment Size	.026 (2.47)	.017 (1.95)	.018 (4.03)
Log Establishment Size GT 50	.008 (1.02)	-.005 (.67)	-.008 (.72)
Proportion Unionized	-.004 (.38)	-.005 (.43)	.251 (14.44)
Proportion White Collar	.027 (2.48)	.025 (2.26)	.108 (7.20)
Proportion Craft	.088 (5.91)	.087 (5.83)	.090 (4.46)
Market Characteristics			
Log Market Wage	.070 (2.91)	.071 (2.95)	.240 (7.02)
Change Market Employment	.205 (2.75)	.210 (2.80)	.352 (3.25)
Tenure Variables			
Tenure ÷ 100	.020 (1.00)	.027 (1.37)	
Tenure ET 6 mo ÷ 100	-.008 (.38)	-.012 (.56)	
Tenure GT 12 mo ÷ 100	.004 (.57)	.002 (.33)	
Lambda	.055 (1.14)	.031 (.69)	
Dummies for Industry	X	X	X
Job Requirements	X	X	X
R Square	.179	.165	.453

establishments with only one employee, 2.2 percent in establishments with 50 employees, and 1.1 percent in establishments with 500 employees.

Some simple models of wage setting predict that the individual's wage should be varied in proportion to his or her relative productivity. Our data suggest that this prediction does not square with reality. If we assume that the index of relative productivity is a proportional transformation of true relative productivity, the implied elasticity of the individual's current wage rate with respect to that person's true productivity is between .09 and .13 in the smallest establishments and correspondingly lower in larger establishments. If our productivity index is a more than proportionate transformation of true productivity (so that it exaggerates the proportionate size of productivity differentials between people and over time), or if the variance of measurement error is large relative to the variance of true productivity, .09 and .13 are downward biased estimates of the true underlying elasticity of current relative wage with respect to current relative productivity in very small firms. It seems unlikely, however, that correcting for these measurement problems would raise the estimate of the true elasticity in very small firms to unity. Even if the smallest firms had a relative wage-relative productivity elasticity of one, large firms would definitely have lower elasticities.

There are a number of plausible explanations for an elasticity of the relative wage with respect to true relative productivity of less than one. First, productivity differentials between workers at a firm might reflect differences in skills that are highly specific to the firm. If the worker is not able to translate high productivity at the current employer into a higher wage offer at another firm, the competitive pressure on the current employer to raise the individual's wage is reduced.

Second, even if all productivity differentials within the firm reflected differences in generalized competence, it is very difficult for other employers to measure these differentials accurately and thus base wage and job offers on them. No one is likely to tell a prospective employer the truth. If the worker is currently employed or on temporary layoff, the individual's employer has a positive incentive to speak very positively about the workers he wants to get rid of and negatively about the workers he wants to keep. Self-reports of one's productivity will also be treated with skepticism.

The only indicator of a worker's relative productivity that is likely to influence another employer is the worker's job classification and relative wage rate. Wage increases and promotions are often justified on the grounds that they will reduce the probability of losing that employee. But they also transmit signals to other employers about the employee's productivity, and consequently, raise the wage the promoted employee is likely to be able to obtain elsewhere. This means that as an instrument for retaining the most productive employees, promotions and wage increases are partially self-defeating. This is the third reason why the elasticity of the relative wage to relative productivity is likely to be less than one.

The fourth explanation is the high cost of accurately measuring a particular worker's productivity. In most jobs, objective indicators of productivity simply do not exist. This is why in November 1975, only 1.2 percent of the nation's workers were paid on a piece rate basis and only 1.9 percent on a pure commission basis (Flaim 1976). In most work environments productivity-based wage setting would have to use subjective evaluations by immediate supervisors. Top managements of large organizations legitimately fear that some line supervisors may abuse the power this kind of wage setting gives them. If a union represents the workers, the ability and inclination of management to adjust wages to productivity is reduced even further. As a result, large organizations greatly restrict the range over which wage rates may be varied. A supervisor's perception of a 50 percent productivity differential may translate into only a 1 or 2 percentage point differential in the wage increase that is awarded. Supervisors may also misperceive the criteria they are supposed to use. In our view, it is the threat of unionization and the difficulty of insuring that supervisors will carry out instructions correctly that are responsible for the very weak connection between relative productivity and relative wage rates in large establishments. In small owner-managed firms, unions are not as much of a threat and the owner makes the decision about the wage to offer. Even if one trusted supervisors to be as fair as possible and employees believed the system was fair, supervisory ratings would not be perfectly correlated with true productivity. Optimal wage setting in such an environment would take into account the measurement error, and the elasticity of the wage rate with respect to measured productivity would be less than one.

The fifth reason for an elasticity below one is random month-to-month or year-to-year variations in productivity. Actual productivity will not be perfectly autocorrelated, so current productivity is an imperfect predictor of next period's productivity. If next period's wages are set equal to next period's expected productivity, the elasticity of the wage with respect to this period's productivity will be less than one.

9.3 The Impact of Workers' Qualifications

Education, experience, age, and being a male all have positive statistically significant effects upon both the level of the starting wage and the rate at which it grows in the first year or so of employment. Controlling for the characteristics of the job, the firm and the reported productivity of the worker reduces but does not eliminate the effects of these characteristics on wage growth.

The small size of the coefficients on our direct measures of productivity leaves room for these proxies for productivity--education, experience, and age--to have direct impacts on wage growth.[1] Our analysis of the determinants of productivity growth showed that both education and experience had small positive impacts on wage growth. How consistent are the coefficients on these productivity proxies in the wage growth equation with the corresponding coefficients in the productivity growth equation? A comparison can be made if we make an assumption about the scaling of the productivity index. Assuming

that it is a proportional transformation of true productivity and a multiplicative error, the change in productivity growth divided by the mean level of productivity is a measure of percentage changes in productivity. The change produced by a year of high school is 1.6 percent, by a year of college is 2.7 percent, by four rather than zero years of relevant experience is 1 percent, by being thirty rather than twenty years old is almost zero (.04 percent). The comparable coefficients in the wage equations are 2.2 percent for high school, 0.04 percent for college, 1.1 percent for four years of relevant experience, and 1.8 percent for being thirty rather than twenty. The effects of experience on the two outcomes were almost identical. The education coefficients seem similar and are not statistically different from each other.

The effect of age is, however, completely different in the two equations. Older workers are not growing more productive any faster than young workers and are not reported to be any more productive at the time of the interview, but they do get considerably larger wage increases and have higher starting wage rates as well. The hypothesis that age coefficients in the wage and productivity growth equations are the same, is rejected by the data ($t = 2.43$ assuming that the error variances of the two equations are uncorrelated). Sex also has dramatically different effects in the two equations. Men get a 6.3 percent greater wage increase than women but they seem to learn less quickly and their index of relative productivity is about 3 percent lower than women at the time of the interview.[2] The difference between the coefficients in the two equations is statistically significant ($t = 6.37$, assuming that error variances of the two equations are uncorrelated).

How much confidence can be placed in these findings? Our results depend upon potentially controversial assumptions about the validity and scaling of the index of relative productivity and on having successfully controlled for the characteristics of the job. The jobs that men and women get are generally quite different and our measures of job requirements may not have controlled for all these differences. Therefore, we do not view our findings as conclusive. They need to be checked in other data sets. Our findings about the effects of age are quite consistent with Medoff and Abraham's (1981a) findings. Our age-at-time-of-hiring variable corresponds closely to their years-of-precompany-experience variable. In all of the data sets they examined, they found that when grade level of the job is held constant, additional years of precompany experience were associated with significantly lower performance ratings and with significantly higher wage rates (p. 200, 201). Thus we feel there is good evidence that part of the tendency of wage rates to rise with age cannot be justified by a corresponding rise of productivity with age. In our data, age is rewarded with higher wage rates even when it is not associated with additional, previous, useful work experience. Additional years of previous, useful job experience are associated with being more productive, but controlling for this experience, age has almost no independent effect on the level of productivity and a negative effect on its rate of change. We have not been able to find any studies that have compared wage and productivity growth of males and females in the same job. More research is required before it can be established whether the effects that seem to be associated with the a job occupant's sex are truly an effect of sex or are actually an effect of

the job (i.e., Do male typists and female construction workers receive different wage rates and promotions than others in their occupation?).

One possible explanation for this pattern may be that older workers and males have more attractive alternative opportunities, so competitive pressure to raise their wages is greater than it is for women and for young workers. Other possible explanations are that (a) men and older workers typically receive training that is more general, (b) their training is completed while the training of women and younger workers is continuing and will result in a catch-up wage increase in the near future, or (c) some firms display a taste for discriminating against women and young workers.

The receipt of a subsidy has no observable impact upon the rate of wage increase. The time that personnel and supervisory staff spend training the individual does seem to be associated with higher rates of wage increase. Twenty extra hours of management training time increase wage growth by 0.62 percent, and the effect is significantly different from zero. Wage increases are not, however, affected by the amount of time that coworkers spend training the new employee. The tendency to reward management training time more than coworker training time is consistent with our earlier finding (in section 8.5) that management training time has a larger per hour impact on productivity growth than coworker training time. While the differences between the coefficients in the wage growth equations are larger, the data would not reject a hypothesis that the ratio of the coefficients in the productivity growth equation is equal to their ratio in the wage growth equation. Another possible explanation of the finding is that managers (the people who set wage scales and make promotion decisions) are more aware of the training that they and their staff provide and, therefore, tend to reward it more than the training provided by coworkers.

9.4 Impacts of Employer Characteristics

The size of the establishment has a large statistically significant effect on the rate at which wage rates increase. Establishments with only two employees typically offer wage increases that are 8.5 percent smaller than establishments with 50 employees. Establishments with 500 employees typically offer wage increases that are 4.2 percent higher than establishments with 50 employees. Here again coefficients on establishment size in the productivity growth equations are similar. Making the standard assumptions about the scaling of the productivity index, the productivity growth at establishments with two employees was 9.7 percent lower than the productivity growth at establishments with 50 employees. Productivity growth at establishments with 500 employees was 1.3 percent higher.

The proportion of the establishment's work force in white-collar occupations and the proportion in craft occupations both had large statistically significant impacts on wage growth. A two standard deviation increase in the proportion in white-collar occupation (a change of .70) raises the starting wage rate by 7.5 percent and the rate of wage growth by 1.9 percent. A two standard deviation increase in the proportion in craft occupations (a change

of .48) raises the starting wage by 4.3 percent and the rate of wage growth by 4.2 percent. Unionization has no effect on the rate of wage growth but does have a large impact on the level of starting wages. Holding the characteristics of the job and the worker constant, starting wage rates at a unionized firm are typically 25 percent higher than those at a nonunion firm.

9.5 Impacts of Market Characteristics

Indicators of the availability and attractiveness of alternative job opportunities in the local labor market had large positive and significant effects on wage growth and on the level of the starting wage. A 10 percent higher local manufacturing wage was associated in our data with a 2.4 percent higher starting wage and a .7 percent larger wage increase. Workers in tight labor markets also get higher wage rates. The best measure of demand pressure in a labor market is the rate of growth of employment in that labor market. In labor markets with yearly growth rates of employment that are 4.1 percent greater than average (a change of two standard deviations), entry-level jobs typically pay 2.9 percent more and early wage increases are 1.7 percent higher.

NOTES

1. If there is no measurement error in our productivity index and if the true elasticity of relative wages with respect to relative productivity is 1, the productivity index should explain much of the within-firm variation of relative wages, and the coefficients on variables such as education, experience, and age should be close to zero.

2. Most men and women work in occupationally segregated jobs, so the referent for our index of relative productivity is generally going to be other workers of the same sex. Under these circumstances it is not clear what a correlation between sex and the level of the index of relative productivity means.

APPENDIX

THIS STUDY IS AUTHORIZED BY SECTION 311 CETA 1973 AS AMENDED 1978.

OMB # 445-79037
EXPIRES: June 30, 1980

EOPP EMPLOYERS SURVEY-WAVE I
MAIN QUESTIONNAIRE

CASE ID# [ ] (1-2) - [ ][ ][ ][ ] (3-6) - [ ][ ] (7-8) - [0][1][1] (9-11)

RESPONDENT NAME: _____

ESTABLISHMENT NAME: _____

OFFICE USE ONLY:	--
SIC CODE	[ ][ ][ ][ ] (12-15)
ESTABLISHMENT WEIGHT	[ ][ ].[ ] (16-17.18)
NUMBER OF EMPLOYEES	[ ][ ][ ][ ][ ] (19-23)
QUESTIONNAIRE DISPOSITION	[ ] (24)
DATE FINALIZED	MONTH [ ][ ] (25-26) DAY [ ][ ] (27-28)
BASIC WEIGHT	[ ][ ][ ].[ ] (29-30-31.32)
BATCH NUMBER	[ ][ ][ ][ ] (33-34-35-36)

INTRODUCTION

Hello, my name is _____ with Westat, a national research company near Washington, D. C. We are conducting a study for the Department of Labor. The study is authorized by Section 311 of the Comprehensive Employment and Training Act of 1973.

The purpose of the study is to measure how government programs and current economic factors are affecting the quality of the labor force and the business environment in your community.

Your voluntary participation in this study will be most appreciated and all information you provide will be kept confidential. The responses you give will be used to prepare statistical totals and will not be identified with you or your organization.

Prepared by:

WESTAT
An Employee-Owned Research Corporation
[illegible]

CALL RECORD					
INT. NAME	DATE	TIME Began	TIME Ended	RESULT*	COMMENTS
1.		am pm	am pm		
2.		am pm	am pm		
3.		am pm	am pm		
4.		am pm	am pm		
5.		am pm	am pm		
6.		am pm	am pm		
7.		am pm	am pm		
8.		am pm	am pm		
9.		am pm	am pm		
10.		am pm	am pm		
11.		am pm	am pm		
12.		am pm	am pm		

*RESULT CODES	
INTERIM	FINAL
M1. NO ANSWER M2. 1st REFUSAL/BREAKOFF M3. BUSY SIGNAL M4. CALLBACK - NO APPOINTMENT M5. CALLBACK - APPOINTMENT M8. OTHER (SPECIFY IN COMMENTS) M9. INFORMATION REQUEST PRIOR TO INTERVIEW (ENTER MAILING INFORMATION ON SCR)	MC - COMPLETE MPC - PARTIAL COMPLETE MRB - REFUSAL/BREAKOFF MNA - NO ANSWER MO - OTHER (SPECIFY IN COMMENTS)

APPOINTMENT RECORD	
1. DATE: _____ TIME: _____ am pm COMMENTS: _____	2. DATE: _____ TIME: _____ am pm COMMENTS: _____
1. DATE: _____ TIME: _____ am pm COMMENTS: _____	2. DATE: _____ TIME: _____ am pm COMMENTS: _____

RESPONDENT GRID		
RESPONDENT NAME	TELEPHONE #	SECTION/QUESTIONS

0450
Version 3
10/29/79

SCREENER

HOPP EMPLOYERS SURVEY

1. Is this (NAME OF ESTABLISHMENT FROM INFORMATION SHEET, ITEM # ____)?

   YES 1 (Q4)

   NO 2 (Q2)

2. Did this establishment ever operate under the name (NAME OF ESTABLISHMENT FROM INFORMATION SHEET, ITEM # ____)?

   YES 1

   NO 2

   DON'T KNOW 8

3. What is the (current) name of this establishment?

   NAME: _____

4. Does this establishment do business under any name(s) other than (NAME OF ESTABLISHMENT FROM Q3)?

   YES 1 (Q5)

   NO . . . . . 2 (BOX A)

   DON'T KNOW . . . . . 8 (BOX A)

5. What (is/are) the other name(s)? (PROBE FOR ALL OTHER NAMES.)

   _____

   _____

   _____

BOX A

INTERVIEWER: CHECK Q2.

- IF Q2 = 2, TERMINATE THE INTERVIEW BY SAYING: I'm sorry. I was trying to contact the (NAME OF ESTABLISHMENT FROM INFORMATION SHEET ITEM # ____). Thank you for your help.
- IF Q2 ≠ 2, CONTINUE WITH Q6.

6. Our records show that this is a (TYPE OF ESTABLISHMENT FROM INFORMATION SHEET ITEM # ____) establishment. Is that correct?

   YES 1 (Q8)

   NO 2 (Q7)

7. What type of establishment is this?

   TYPE OF ESTABLISHMENT: _____

8. In what county is this establishment located?

   COUNTY: _____

Does your company have any establishments located in (TARGET AREA)?

YES. 1 (Q10)

NO . . . . . 2 (BOX C)

BOX C

INTERVIEWER:
- OBTAIN THE NAME OF THE R. AND RECORD IT ON THE INFORMATION SHEET.
- TERMINATE THE INTERVIEW BY SAYING: I'm sorry, there seems to be a problem with my information. I need to check with my supervisor but I may be calling you back. Thank you.

10. Is the hiring for this establishment done at this location?

YES. 1

NO 2

DON'T KNOW 8

11. Is the hiring done at this establishment location for any other establishment?

YES. 1

NO 2

DON'T KNOW 8

12. Who would be the best person in your establishment to speak to about personnel and hiring practices? (OBTAIN NAME, MAILING ADDRESS AND PHONE NUMBER OF CONTACT PERSON).

NAME: _____

ADDRESS: _____

_____

_____

PHONE #: ( ) -

BOX D

INTERVIEWER CHECK INFORMATION SHEET:
- IF COMPANY HAS ONLY ONE SAMPLED ESTABLISHMENT IN (TARGET AREA) ATTEMPT TO CONDUCT INTERVIEW WITH PERSON IN Q12.
- IF COMPANY HAS MORE THAN ONE SAMPLED ESTABLISHMENT IN (TARGET AREA) ASK Q13 OF PERSON LISTED IN Q12.

13. I need a little information about the following establishments of yours located in (TARGET AREA) to determine who I need to talk to. (LIST SAMPLED ESTABLISHMENTS.) Is there one central location where I could obtain information about hiring and accounting procedures for all of these establishments?

YES. 1 (Q14)

NO 2 (Q15)

14. Please give me the name and address of this central location and the name and phone number of the person most familiar with hiring and accounting information.

LOCATION NAME AND ADDRESS	CONTACT PERSON NAME AND PHONE NUMBER

THANK THE R. AND END THE INTERVIEW.

15. Could you tell me the name and mailing address of each of your establishments located in (TARGET AREA)? (ENTER INFORMATION IN SPACE PROVIDED BELOW.)

NAME AND ADDRESS OF ESTABLISHMENT

A. _____

B. _____

C. _____

D. _____

E. _____

16. Approximately how many employees work at (NAME OF ESTABLISHMENT)? (RECORD # OF EMPLOYEES FOR EACH ESTABLISHMENT LISTED IN Q.15).

OF EMPLOYEES

A. _____

B. _____

C. _____

D. _____

E. _____

17. Please give me the name and phone number of the person most familiar with (hiring/accounting) at (NAME OF ESTABLISHMENT).

CONTACT PERSON(S) - NAME AND PHONE NUMBER

A. HIRING: _____

ACCOUNTING: _____

B. HIRING: _____

ACCOUNTING: _____

C. HIRING: _____

ACCOUNTING: _____

D. HIRING: _____

ACCOUNTING: _____

E. HIRING: _____

ACCOUNTING: _____

BOX E

INTERVIEWER: INFORM R. THAT CONTACT PERSON(S) LISTED IN Q17 WILL BE RECEIVING A DESCRIPTION OF THE TYPES OF INFORMATION WE NEED AND THAT SOMEONE WILL BE CALLING BACK IN ABOUT TWO WEEKS TO COLLECT THE INFORMATION BY PHONE.

TIME BEGAN: _____ AM
PM

SECTION A - EMPLOYMENT STATISTICS

BOX A

INTERVIEWER:

(1) RECORD NUMBER OF ESTABLISHMENTS COVERED BY QUESTIONNAIRE: _____

(2) RECORD COUNTIES WHERE ESTABLISHMENTS ARE LOCATED: _____

_____

_____

This first series of questions concerns information on general hiring practices, as well as information on the number of employees [your establishment/the (NUMBER) establishment(s) in (TARGET AREA)] employ(s) and the number of employees hired and terminated from October through December 1979. You will probably want to refer to your records for some of the questions.

1. How many employees both full and part time did (this/these) establishment(s) employ during the week of December 12, 1979?

37 38 39 40 41

ZERO EMPLOYEES . . . . . 99997 (EXT. SHEET A)

OR

NUMBER OF EMPLOYEES: _____ (Q2)

2. About how many employees did (this/these) establishment(s) employ on July 1, 1979?

EMPLOYEES: _____

42 43 44 45 46

3. Generally speaking, how difficult or easy would you say it is to find reliable unskilled workers at "reasonable" wages (at that location)? Would you say it is:

very difficult, ... 1
somewhat difficult, ... 2
not very difficult, or 3
easy? 4
NOT APPLICABLE. 5
DON'T KNOW. 8

47

4. During the past year, have any job openings been listed with the State Employment Service, that is the Job Service for (this/these) establishment(s)?

YES. 1
NO 2
DON'T KNOW 8

48

5. During October through December 1979, about how many phone calls did you (or did your personnel office) receive during an average week from people seeking work? Do not include calls from employment agencies.

CALLS: _____

49 50 51

6. During October through December 1979, about how many people came to (this/these) establishment(s) looking for work?

PEOPLE: _____ (Q7)

OR

NONE 997 (Q8)

52 53 54

7. Of those, how many completed written applications for employment?

PEOPLE: _____

55 56 57

8. From October through December 1979, how many employees were newly hired at (this/these) establishment(s)? New hires are permanent, temporary or seasonal employees who have never before been employed by the organization. [ ][ ][ ] 58 59 60

ONE EMPLOYEE. 001 (Q9)
OR
NUMBER OF EMPLOYEES: _____ (Q10)
NONE. 997 (Q11)

9. Is this person still with the organization?

YES 1 (Q11)
NO. . . . . 2 (Q11) [ ] 61

10. How many of these employees are still with the organization?

EMPLOYEES: _____ [ ][ ][ ] 62 63 64

11. From October through December 1979, how many employees were rehired at (this/these) establishment(s)? Rehires are permanent, temporary or seasonal employees who have worked for the organization sometime in the past. [ ][ ][ ] 65 66 67

ONE EMPLOYEE. 001 (Q12)
OR
NUMBER OF EMPLOYEES: _____ (Q13)
NONE. 997 (Q14)

12. Is this person still with the organization?

YES 1 (Q14)
NO. . . . . 2 (Q14) [ ] 68

13. How many of these employees are still with the organization?

EMPLOYEES: _____ [ ][ ][ ] 69 70 71

14. Did any employees separate from (this/these) establishment(s) from October through December 1979?

YES 1 (Q15)
NO. . . . . 2 (SECTION B) [ ] 72

15. How many permanent or temporary employees were fired from October through December 1979? (By fired we mean, a termination initiated by the employer for reasons such as incompetence, absenteeism, or insubordination.)

EMPLOYEES: _____ [ ][ ][ ][ ] 73 74 75 76

16. How many permanent employees were laid off during that period? (By laid off we mean, a suspension from pay status for more than 7 days initiated by the employer without prejudice to the worker for reasons such as: lack of work or materials, model changeover, or plant breakdown, etc.)

CARD [0][2][1] 9 10 11

ONE EMPLOYEE. 0001 (Q17)
OR
NUMBER OF EMPLOYEES: _____ (Q18)
NONE. . . . . 9997 (Q19) [ ][ ][ ][ ] 12 13 14 15

17. Would this employee have been retained had (she/he) been doing a better job?

YES 1 (Q19)
NO. . . . . 2 (Q19) [ ] 16

18. How many of these permanent employees who were laid off would have been retained had they been doing a better job?

EMPLOYEES: _____ 17 18 19

19. How many temporary or seasonal employees were let go during that period? 20 21 22 23

ONE EMPLOYEE. 0001 (Q20)
OR
NUMBER OF EMPLOYEES: _____ (Q21)
NONE. 9997 (Q20)

20. Would this employee have been retained had (he/she) been doing a better job? 24

YES. 1 (Q22)
NO 2 (Q22)

21. How many of these temporary or seasonal employees who were let go would have been retained had they been doing a better job? 25 26 27

EMPLOYEES: _____

22. How many permanent or temporary employees quit during that period? (By quit we mean separations initiated by the employee for any reason except retirement, transfer to another establishment in your company or service in the Armed Forces?)

ONE EMPLOYEE. 001 (Q23)
OR
NUMBER OF EMPLOYEES: _____ (Q24) 28 29 30
NONE. . . . . 997 (SECTION B)

23. Did this person quit in anticipation of being discharged or because supervisors had expressed dissatisfaction with his or her job performance? 31

YES. . . . . 1 (SECTION B)
NO . . . . . 2 (SECTION B)

24. Of these employees, how many quit in anticipation of being discharged or because supervisors had expressed dissatisfaction with their job performance? 32 33

INDUCED QUITS: _____

SECTION B - GOVERNMENT PROGRAMS

Now I'd like to ask you some questions regarding a few government programs.

25. Are you familiar with the New Jobs Tax Credit?

YES	1 (Q26)	
NO	2 (Q28)	
DON'T KNOW	8 (Q28)	☐ 34

26. Did the New Jobs Tax Credit influence (this/any of these) establishment(s) to increase employment more than it otherwise would have done?

YES	1	
NO	2	
DON'T KNOW	8	☐ 35

27. Did you receive a New Jobs Tax Credit for expanding employment during 1977 or 1978 for (this/any of these) establishment(s)?

YES	1	
NO	2	☐ 36

28. Are you familiar with the Targeted Job Tax Credit?

YES	1 (Q29)	
NO	2 (Q32)	
DON'T KNOW	8 (Q32)	☐ 37

29. Did you learn about the Targeted Jobs Tax Credit Program before September, 1979?

YES	1	
NO	2	
DON'T KNOW	8	☐ 38

30. (Has this/Have any of these) establishment(s) ever hired any employees through the Targeted Job Tax Credit Program?

YES	1 (Q31)	
NO	2 (Q32)	
DON'T KNOW	8 (Q32)	☐ 39

31. Did you receive a tax credit for any employee hired through the Targeted Job Tax Credit Program?

YES	1	
NO	2	☐ 40

32. Are you familiar with WIN, that is the Work Incentive Tax Credit Program?

YES	1 (Q33)	
NO	2 (Q37)	
DON'T KNOW	8 (Q37)	☐ 41

33. Did you learn about the WIN program before September, 1979?

YES	1	
NO	2	
DON'T KNOW	8	☐ 42

34. How did you first learn about the WIN program?

JOB APPLICANT	1
GOVERNMENT REPRESENTATIVE	2
REPRESENTATIVE OF A LOCAL BUSINESS ORGANIZATION	3
GENERAL WORD OF MOUTH	4
THROUGH THE MEDIA (TV, RADIO, NEWSPAPERS, LITERATURE)	5
OTHER (SPECIFY) _____	6
DON'T KNOW	8

43

35. Did (this/any of these) establishment(s) hire any employees through the WIN Program in 1977? In 1978? In 1979?

44 45 46

	1977	1978	1979
YES	1	1	1
NO	2	2	2
DON'T KNOW	8	8	8

BOX B

INTERVIEWER: REVIEW Q35 AND CIRCLE ONE

AT LEAST ONE YEAR = YES 1 (Q36)
ALL YEARS = NO/DK 2 (Q37)

47

36. In (YEAR), did you receive a tax credit for any employee hired through WIN? (ALL YEARS IN Q35 THAT = NO/DK SHOULD BE CODED NA.)

48 49 50

	1977	1978	1979
YES	1	1	1
NO	2	2	2
NOT APPLICABLE	3	3	3
DON'T KNOW	8	8	8

37. Are you familiar with any CETA program or any other programs sponsored by (CARD SERIES A)?

READ CARD A

YES	1 (Q38)
NO	2 (BOX D)
DON'T KNOW	8 (BOX D)

51

38. Did you learn about the CETA program before September, 1979?

YES	1
NO	2
DON'T KNOW	8

52

39. How did you first learn about the CETA program?

JOB APPLICANT	1
GOVERNMENT REPRESENTATIVE	2
REPRESENTATIVE OF A LOCAL BUSINESS ORGANIZATION	3
GENERAL WORD OF MOUTH	4
THROUGH THE MEDIA (TV, RADIO, NEWSPAPERS, LITERATURE)	5
OTHER (SPECIFY) _____	6
DON'T KNOW	8

53

40. Did (this/any of these) establishment(s) hire any employees through a CETA program in 1977? In 1978? In 1979?

54 55 56

	1977	1978	1979
YES	1	1	1
NO	2	2	2
DON'T KNOW	8	8	8

BOX C

INTERVIEWER: REVIEW Q40 AND CIRCLE ONE

AT LEAST ONE YEAR = YES 1 (Q41)

ALL YEARS = NO/DK 2 (Q43)

57

41. In (YEAR), did you receive a subsidy for any employee hired through CETA? (ALL YEARS IN Q40 THAT = NO/DK SHOULD BE CODED NA)

58 59 60

	1977	1978	1979
YES	1	1	1
NO	2	2	2
NOT APPLICABLE	3	3	3
DON'T KNOW	8	8	8

42. In (LATEST YES YEAR IN Q40), how many hours did employees in your organization spend negotiating, and fulfilling the paperwork requirements of the contract with the local CETA agency for the CETA employees in (this/all of these) establishment(s)? (PROBE FOR ESTIMATE)

61 62 63

HOURS: _____ (BOX D)

43. Imagine you had a contract with your local CETA agency to train one employee. How many total hours do you think employees in your organization would have to spend negotiating and fulfilling the paperwork requirements of such a contract?

64 65 66

HOURS: _____

DON'T KNOW 998

BOX D

INTERVIEWER: REVIEW Q30, Q35 AND Q40 AND CIRCLE ONE

PARTICIPATED IN PROGRAM (Q30 OR 35 OR 40 = YES) 1 (Q44)

DID NOT PARTICIPATE (ALL Qs = NO/DK). . . . . 2 (SECTION C)

67

44. Did the participation in the programs we just talked about, influence (this/these) establishment(s) to expand total employment by more than might otherwise have been done?

YES 1 (Q45)

NO 2 (Q46)

68

45. During the most recent year that (this/any of these) establishment(s) participated in a government program, approximately how many additional employees were hired that wouldn't have been hired otherwise?

69 70 71

EMPLOYEES: _____

DON'T KNOW 998

46. During the most recent year that (this/any of these) establishment(s) participated in a government program, what was the total number of employees for which a tax credit or a subsidy was received?

ONE EMPLOYEE. 001 (Q47) 72 73 74

OR

NUMBER OF EMPLOYEES: _____ (Q48)

NONE. . . . . 997 (SECTION C)

47. Is this employee still with the organization?

YES. . . . . 1 (SECTION C)

NO . . . . . 2 (SECTION C) 75

48. How many of these employees are still with the organization?

NUMBER OF EMPLOYEES: _____ 76 77 78 79

SECTION C - EMPLOYEE QUESTIONS

CARD 0 3 I (9 10 11)

The next series of questions concern employees that were hired between January 1, 1978 and October 1, 1979. Are you familiar with personnel matters during that time period, or should I speak to someone else? (IF NEW R, RECORD NAME AND PHONE NUMBER IN R GRID)

BOX I
CURRENT R CAN ANSWER SECTION C. 1 (Q49)
CURRENT R CANNOT ANSWER SECTION C . . . . . 2 (SECTION D)

12

49. First I'd like to ask you about the last (CARD SERIES B) hired who was not subsidized or for whom you did not receive tax benefits. I am only interested in the most recent newly hired person, not in a rehired person.

READ CARD B

Did you hire anyone like this for (this/any of these) establishment(s) between January 1, 1978 and October 1, 1979?

YES. 1 (Q52)
NO 2 (Q50)

13

50. Now, I'd like to ask you about the last (CARD SERIES B) hired for whom a government tax benefit or subsidy was received.

Did you hire anyone like this for (this/any of these) establishment(s) between January 1, 1978 and October 1, 1979?

YES. 1 (Q51)
NO . . . . . 2 (SECTION D)

14

51. Which government program provided the tax benefit or subsidy for this employee?

PROGRAM: _____ ___ (Q52)
DON'T KNOW 98 (Q52)

15 16

ASK Q52 - Q64 FOR THE NONSUBSIDIZED WORKER (COL. 1) AND THEN FOR THE SUBSIDIZED WORKER (COL. 2)	NONSUBSIDIZED COL. 1	SUBSIDIZED COL. 2
52. What is the first name of the last (nonsubsidized/subsidized) (CARD SERIES B) hired prior to October 1, 1979? (I need the first name to make it easier to refer to the person during the next few questions.)	_____ FIRST NAME	_____ FIRST NAME
53. (INTERVIEWER: ASK IF NOT OBVIOUS) Is (NAME) a male or a female?	MALE..... 1 FEMALE..... 2 17	MALE..... 1 FEMALE..... 2 24
54. In what month and year did (NAME) begin working?	___/___ MO YR 18 19 20	___/___ MO YR 25 26 27
55. Approximately, how long was it between the time you started to recruit for the job and the time (NAME) started work?	_____ TIME CIRCLE ONE: DAYS..... 1 (Q56) WEEKS..... 2 (Q56) MONTHS..... 3 (Q56) ALWAYS LOOKING.... 996 (Q56) DID NOT RECRUIT.... 997 (Q61) 21 22 23	_____ TIME CIRCLE ONE: DAYS..... 1 (Q56) WEEKS..... 2 (Q56) MONTHS..... 3 (Q56) ALWAYS LOOKING.... 996 (Q56) DID NOT RECRUIT.... 997 (Q61) 28 29 30

ASK Q52 - Q64 FOR THE NONSUBSIDIZED WORKER (COL. 1) AND THEN FOR THE SUBSIDIZED WORKER (COL. 2)	NONSUBSIDIZED COL. 1	SUBSIDIZED COL. 2
56. How many job applicants, including (NAME) were interviewed for the position?	ONE APPLICANT..01 (Q59) OR NUMBER OF APPLICANTS____ (Q57) 31 32	ONE APPLICANT..01 (Q59) OR NUMBER OF APPLICANTS____ (Q57) 48 49
57. Did you offer this job to anyone else who turned it down?	YES..... 1 (Q58) NO..... 2 (Q59) 33	YES..... 1 (Q58) NO..... 2 (Q59) 50
58. How many people turned it down?	PEOPLE: ____ 34 35	PEOPLE: ____ 51 52
59. [Of those interviewed for the position, how many were/Was (NAME)] referred to you by the Job Service, CETA, or any other government program?	____ REFERRALS 36 37	____ REFERRALS 53 54
60. Approximately how many hours did you and other staff spend recruiting, screening and interviewing the applicant(s) for this position? (IF DAYS, ETC., CONVERT TO HOURS)	____ HOURS 38 39 40	____ HOURS 55 56 57
61. How many months or years of useful job experience did (NAME) have, before (he/she) started working with your establishment?	MOS. ____ OR YRS. ____ NONE..... 997 DON'T KNOW..... 998 41 42 43	MOS. ____ OR YRS. ____ NONE..... 997 DON'T KNOW..... 998 58 59 60
62. Approximately how old was (NAME) when you hired (him/her)?	____ AGE DON'T KNOW..... 98 44 45	____ AGE DON'T KNOW..... 98 61 62
63. Approximately how much schooling had (NAME) completed when you hired (him/her)?	NO FORMAL SCHOOLING..... 1 GRADE SCHOOL (GRADES 1-8)..... 2 SOME HIGH SCHOOL..... 3 HIGH SCHOOL GRADUATE..... 4 SOME COLLEGE..... 5 COLLEGE GRADUATE..... 6 OTHER (SPECIFY) ____ 7 DON'T KNOW..... 8 46	NO FORMAL SCHOOLING..... 1 GRADE SCHOOL (GRADES 1-8)..... 2 SOME HIGH SCHOOL..... 3 HIGH SCHOOL GRADUATE..... 4 SOME COLLEGE..... 5 COLLEGE GRADUATE..... 6 OTHER (SPECIFY) ____ 7 DON'T KNOW..... 8 63
64. Is (NAME) still with the establishment?	YES..... 1 (Q74) NO..... 2 (Q65) 47	YES..... 1 (Q74) NO..... 2 (Q65) 64

ASK Q52 - Q84 FOR THE NONSUBSIDIZED WORKER (COL. 1) AND THEN FOR THE SUBSIDIZED WORKER (COL. 2)	NONSUBSIDIZED COL. 1	SUBSIDIZED COL. 2
65. Was the separation a layoff, a discharge, an induced resignation, or a voluntary resignation? (People are "induced to resign" primarily because they anticipated they would be discharged or because supervisors had expressed dissatisfaction with their performance.)	LAYOFF..... 1 DISCHARGE..... 2 INDUCED RESIGNATION..... 3 VOLUNTARY RESIGNATION..... 4 OTHER..... 5 DON'T KNOW..... 8 □ 65	LAYOFF..... 1 DISCHARGE..... 2 INDUCED RESIGNATION..... 3 VOLUNTARY RESIGNATION..... 4 OTHER..... 5 DON'T KNOW..... 8 □ 66
66. How long did (NAME) stay with the establishment?	_____ TIME CIRCLE ONE: DAYS..... 1 WEEKS..... 2 MONTHS..... 3 YEARS..... 4 □□□ 12 13 14	_____ TIME CIRCLE ONE: DAYS..... 1 WEEKS..... 2 MONTHS..... 3 YEARS..... 4 □□□ 33 34 35
BOX F INTERVIEWER: REVIEW Q66 AND CIRCLE ONE: PERSON STAYED 1 MO. OR MORE. . . . . . PERSON STAYED LESS THAN 1 MONTH.	1 (Q70) 2 (Q67) □ 15	1 (Q70) 2 (Q67) □ 36
67. Approximately how many hours did employees other than personnel and supervisory staff, spend away from their normal work routines orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 □□ 16 17	_____ HOURS DON'T KNOW..... 98 □□ 37 38
68. Approximately how many hours did personnel and supervisory staff spend orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 □□ 18 19	_____ HOURS DON'T KNOW..... 98 □□ 39 40
69. If you consider the productivity of an average experienced worker in this job to be 50 on a scale from 1 to 100, what rating would you give (NAME) for (his/her) productivity while (he/she) worked for you? (PROBE FOR ESTIMATE)	_____ (Q80) □□□ 20 21 22	_____ (Q80) □□□ 41 42 43
70. In the first month of (NAME)'s employment, approximately how many hours did employees other than personnel and supervisory staff, spend away from their normal work routines orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 □□ 23 24	_____ HOURS DON'T KNOW..... 98 □□ 44 45
71. In the first month of (NAME)'s employment, approximately how many hours did personnel and advisory staff spend orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 □□ 25 26	_____ HOURS DON'T KNOW..... 98 □□ 46 47
72. If you consider the productivity of an average experienced worker in this job to be 50 on a scale from 1 to 100, what rating would you give (NAME) for (his/her) productivity during (his/her) 2nd week of employment? (PROBE FOR ESTIMATE)	_____ □□□ 27 28 29	_____ □□□ 48 49 50
73. Again, considering the productivity of an average experienced worker to be 50 on a scale from 1 to 100, what rating would you give (NAME) for (his/her) productivity during the week prior to (his/her) separation? (PROBE FOR ESTIMATE)	_____ (Q80) □□□ 30 31 32	_____ (Q80) □□□ 51 52 53

CARD
0 4 I
9 10 11

ASK Q52 - Q84 FOR THE NONSUBSIDIZED WORKER (COL. 1) AND THEN FOR THE SUBSIDIZED WORKER (COL. 2)	NONSUBSIDIZED COL. 1	SUBSIDIZED COL. 2
74. In the first month of (NAME)'s employment, approximately how many hours did employees other than personnel and supervisory staff, spend away from their normal work routines orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 54 55	_____ HOURS DON'T KNOW..... 98 58 59
75. In the first month of (NAME)'s employment, approximately how many hours did personnel and supervisory staff spend orienting and training (NAME)?	_____ HOURS DON'T KNOW..... 98 56 57	_____ HOURS DON'T KNOW..... 98 60 61
76. If you consider the productivity of an average experienced worker in this job to be 50 on a scale from 1 to 100, what rating would you give (NAME) for (his/her) productivity during (his/her) 2nd week of employment? (PROBE FOR ESTIMATE)	_____ 12 13 14	_____ 34 35 36
77. Again considering the productivity of an average experienced worker to be 50, what rating would you give (NAME) at this time? (PROBE FOR ESTIMATE)	_____ AND CIRCLE ONE: LESS THAN 50..... 1 (Q78) 50 OR MORE.... 2 (Q79) 15 16 17	_____ AND CIRCLE ONE: LESS THAN 50..... 1 (Q78) 50 OR MORE.... 2 (Q79) 37 38 39
78. When do you expect (NAME) to equal the productivity of an average worker in this job? (PROBE FOR ESTIMATE)	_____ TIME CIRCLE ONE: DAYS..... 1 (Q80) WEEKS..... 2 (Q80) MONTHS..... 3 (Q80) YEARS..... 4 (Q80) NEVER..... 997 (Q80) DON'T KNOW.. 998 (Q80) 18 19 20	_____ TIME CIRCLE ONE: DAYS..... 1 (Q80) WEEKS..... 2 (Q80) MONTHS..... 3 (Q80) YEARS..... 4 (Q80) NEVER..... 997 (Q80) DON'T KNOW.. 998 (Q80) 40 41 42
79. Approximately how long did it take for (NAME) to reach the productivity of the average worker in this job? (PROBE FOR ESTIMATE)	_____ TIME CIRCLE ONE: DAYS..... 1 WEEKS..... 2 MONTHS..... 3 YEARS..... 4 DON'T KNOW..... 998 21 22 23	_____ TIME CIRCLE ONE: DAYS..... 1 WEEKS..... 2 MONTHS..... 3 YEARS..... 4 DON'T KNOW..... 998 43 44 45
80. What was the title of (NAME)'s job at the time (he/she) was hired? What were (his/her) most important duties? (PROBE)	TITLE: _____ DUTIES: _____ 24 25 26 27 28 29	TITLE: _____ DUTIES: _____ 46 47 48 49 50 51
81. What is the current starting wage of this job?	$ _____ PER HOUR..... 1 WEEK..... 2 MONTH..... 3 YEAR..... 4 30 31 32 33	_____ PER HOUR..... 1 WEEK..... 2 MONTH..... 3 YEAR..... 4 52 53 54 55

CARD 0 5 I
9 10 11

ASK Q82 - Q84 FOR THE NONSUBSIDIZED WORKER (COL. 1) AND THEN FOR THE SUBSIDIZED WORKER (COL. 2)	NONSUBSIDIZED COL. 1	SUBSIDIZED COL. 2
Q82. What is (NAME)'s current wage?	$ _____ PER HOUR..... 1 WEEK..... 2 MONTH..... 3 YEAR..... 4 56 57 58 59	$ _____ PER HOUR..... 1 WEEK..... 2 MONTH..... 3 YEAR..... 4 68 69 70 71
Q83. What is the top of the wage scale for this job?	$ _____ PER HOUR..... 1 (BOX G) WEEK..... 2 (BOX G) MONTH..... 3 (BOX G) YEAR..... 4 (BOX G) NO SET AMOUNT..9996 (Q50) 60 61 62 63	$ _____ PER HOUR..... 1 (BOX G) WEEK..... 2 (BOX G) MONTH..... 3 (BOX G) YEAR..... 4 (BOX G) NO SET AMOUNT..9996 (SEC D) 72 73 74 75
BOX G INTERVIEWER: REVIEW Q81 AND Q83 AND CIRCLE ONE AMOUNT IN Q81 AND Q83 IS SAME..... AMOUNT IN Q81 AND Q83 IS NOT SAME.....	..... 1 (Q50) 2 (Q84) 64	..... 1 (SEC D) 2 (Q84) 76
Q84. Normally how long does it take for a new employee to reach the top of the wage scale for this job?	_____ TIME CIRCLE ONE: DAYS..... 1 (Q50) WEEKS..... 2 (Q50) MONTHS..... 3 (Q50) YEARS..... 4 (Q50) 65 66 67	_____ TIME CIRCLE ONE: DAYS..... 1 WEEKS..... 2 MONTHS..... 3 YEARS..... 4 77 78 79

CARD

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9	10	11

SECTION D - CLASSIFICATION OF WORK FORCE

This last section contains questions on the numbers of people you employ in different job classifications. We would like you to refer to your payroll records for these questions. I would like to see if we can complete these questions now. However if as we go through the next few questions, you feel it would be easier for you to classify your work force while looking at the questions, we will send you a worksheet which you can complete and we will recontact you in about 2 weeks to finish the interview. Shall we try to finish the interview now? (IF R WANTS WORKSHEET MAILED, FILL OUT MAILING INFO ON SCR AND TERMINATE)

85. Was the pay period that included December 12, 1979 a relatively normal one for (this/these) establishment(s)?

YES 1 (Q86)
NO. 2 (Q87)

[ ] 12

86. What was the starting date and the ending date of that pay period?

STARTING DATE: ____/____ MO DY

AND

ENDING DATE: ____/____ MO DY (BOX H)

87. What was the starting date and the ending date of the first pay period after the one including December 12 that you consider relatively normal? (IF NONE NORMAL ASK Q88 OF DECEMBER 12 PAY PERIOD)

[ ][ ][ ][ ] 13 14 15 16

STARTING DATE: ____/____ MO DY

[ ][ ][ ][ ] 17 18 19 20

AND

ENDING DATE: ____/____ MO DY

BOX H

INTERVIEWER: READ THE FOLLOWING

Please use the pay period starting on (DATE) and ending on (DATE) as your reference pay period for the next series of questions.

88. I would like you to classify the permanent, temporary and seasonal employees employed by (this/these) establishment(s) into six job categories. Each category will need to be divided between full-time and part-time employees. The six categories are:

- Managerial, Administrative, Professional and Technical Workers;
- Sales Workers;
- Office and Clerical Workers;
- Craft Workers;
- Operatives; and
- Laborers and Service Workers.

BOX I
INTERVIEWER: ASK Q89 AND Q90 FOR ONE JOB CATEGORY, BEFORE GOING TO THE NEXT JOB CATEGORY.

89. How many full-time (JOB CATEGORY) did (this/these) establishment(s) have during the reference pay period? That would include: (CARD SERIES C) (RECORD BELOW)

READ CARD C

90. How many part-time (JOB CATEGORY) did (this/these) establishment(s) have during the reference pay period? (RECORD BELOW)

91. (FOR PART-TIME EMPLOYEES, ASK:) How many hours per week did the typical part-time (JOB CATEGORY) work during the reference pay period? (RECORD BELOW)

FORM A - TOTAL EMPLOYEES			
JOB CATEGORY	(Q89) FULL-TIME EMPLOYEES	(Q90) PART-TIME EMPLOYEES	(Q91) PART-TIME HOURS PER WEEK
MANAGERIAL, ADMINISTRATIVE, PROFESSIONAL, AND TECHNICAL EMPLOYEES	_____	_____ (Q91)	_____
SALES WORKERS	_____	_____ (Q91)	_____
OFFICE AND CLERICAL WORKERS	_____	_____ (Q91)	_____
CRAFT WORKERS	_____	_____ (Q91)	_____
OPERATIVES	_____	_____ (Q91)	_____
LABORERS AND SERVICE WORKERS	_____	_____ (Q91)	_____
TOTAL			

92. How many hours per week did the typical full-time employee work during the reference pay period?

HOURS: _____ ☐☐

93. I would like you to classify the employees who earn $6.00 an hour or less by job title. However, if it would be easier to classify them by wage grades you can do that. Which would be easier for you?

JOB TITLE. 1
WAGE GRADES. 2

CARD 0 7 1
9 10 11

BOX J
INTERVIEWER: RECORD Q94 - Q100 IN FORM B.

12

94a. (IF JOB TITLES) Starting with (LOWEST JOB CATEGORY), what are the job titles of employees earning $6.00 an hour or less? (REPEAT FOR EACH JOB CATEGORY) (RECORD BELOW)

94b. (IF WAGE GRADES) What are the wage grades of employees earning $6.00 an hour or less? (RECORD BELOW)

BOX K
INTERVIEWER: ASK Q95 AND Q96 FOR ONE JOB TITLE/WAGE GRADE BEFORE GOING TO THE NEXT CATEGORY

95. How many full-time (JOB TITLE/WAGE GRADES) who earn $6.00 an hour or less did (this/these) establishment(s) have during the reference pay period? (REPEAT FOR EACH JOB TITLE/WAGE GRADE)

96. How many part-time (JOB TITLE/WAGE GRADES) who earn $6.00 an hour or less did (this/these) establishments have during the reference pay period? (REPEAT FOR EACH JOB TITLE/WAGE GRADE)

13 14

FORM B - EMPLOYEES PAID $6.00 AN HOUR OR LESS			
(Q94a)	(Q94b)	(Q95)	(Q96)
JOB TITLE	WAGE GRADE	FULL-TIME	PART-TIME
LABORERS AND SERVICE WORKERS: 1. _____ 2. _____ 3. _____	1. _____ 2. _____ 3. _____	_____ _____ _____	_____ _____ _____
OPERATIVES: 1. _____ 2. _____ 3. _____	4. _____ 5. _____ 6. _____	_____ _____ _____	_____ _____ _____
CRAFT WORKERS: 1. _____ 2. _____ 3. _____	7. _____ 8. _____ 9. _____	_____ _____ _____	_____ _____ _____
OFFICE & CLERICAL WORKERS: 1. _____ 2. _____ 3. _____	10. _____ 11. _____ 12. _____	_____ _____ _____	_____ _____ _____
SALES WORKERS: 1. _____ 2. _____ 3. _____	13. _____ 14. _____ 15. _____	_____ _____ _____	_____ _____ _____
MANAGERIAL, ADMINISTRATIVE, PROFESSIONAL, TECHNICAL EMPLOYEES 1. _____ 2. _____ 3. _____	16. _____ 17. _____ 18. _____	_____ _____ _____	_____ _____ _____

BOX L
INTERVIEWER: USE EXTENSION SHEETS IF NECESSARY.

97. During the reference pay period, what was the highest straight time wage excluding fringe benefits for (JOB TITLE/WAGE GRADE)? What was the lowest straight time wage? (REPEAT FOR EACH JOB TITLE/WAGE GRADE) (RECORD BELOW)

BOX M
INTERVIEWER: ASK Q98 - Q100 FOR ONE (JOB TITLE/WAGE GRADE) BEFORE GOING TO THE NEXT.

98. Were any employees earning $6.00 an hour or less, tipped or paid on an incentive basis during the reference pay period? (If YES: which ones? RECORD BELOW)

99. (IF TIPPED OR PAID ON INCENTIVE) How many (JOB TITLE/WAGE GRADE) were tipped or paid on an incentive basis during the reference pay period? (RECORD BELOW)

100. (IF TIPPED OR PAID ON INCENTIVE) On the average, how much extra per hour, did the typical (JOB TITLE/WAGE GRADE) make during the reference pay period? (RECORD BELOW)

	(Q97) STRAIGHT TIME WAGE		(Q98)	(Q99)	(Q100)
	HIGHEST	LOWEST	TIPPED INCENTIVE	NUMBER TIPPED	$ PER HOUR
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____
	$ _____	$ _____	YES..... 1 (Q99)	_____	$ _____
	$ _____	$ _____	NO..... 2 (Q101)	_____	$ _____
	$ _____	$ _____		_____	$ _____

101. Roughly what percentage of your non-supervisory workers are covered by collective bargaining agreements?

PERCENTAGE _____ [ ][ ][ ] 15 16 17
DON'T KNOW 998

102. (Does this/Do these) establishment(s) currently have any job vacancies? By that we mean jobs that are immediately available for filling and for which you have actively tried to find or recruit people from outside your firm?

YES. 1 (Q103)
NO . . . . . 2 (Q104) [ ] 18

103. How many vacancies (does this/do these) establishment(s) have in each of the following job categories? (Please exclude jobs to be filled by recall, transfer, promotion, demotion or return from unpaid leave; jobs unoccupied because of labor management disputes, job openings for which "new" workers were already hired and scheduled to start work later, and the openings with future starting dates.) The categories are:

[ ][ ] 19 20

	Vacancies	
Sales Workers.	_____	[ ][ ] 21 22
Office & Clerical Workers. .	_____	[ ][ ] 23 24
Craft Workers.	_____	[ ][ ] 25 26
Operatives	_____	[ ][ ] 27 28
Laborers & Service Workers .	_____	

104. Do you calculate an average hourly wage for any portion of your work force in (this/these) establishment(s)?

YES. 1 (Q105)
NO . . . . . 2 (BOX N) [ ] 29

105. During the pay period (Q86/87) to (Q86/87), what was the average hourly wage not including benefits?

[ ][ ][ ][ ] 30 31 32 33
AVERAGE HOURLY WAGE: _____
DON'T KNOW 9998

106. What group of workers does this average hourly wage represent?

WORKERS: _____ [ ][ ][ ] 34 35 36

BOX N
INTERVIEWER: CHECK INDUSTRY SIC AND CIRCLE ONE
SIC IS 15-17, 412, 535, 64-66 . . . . . 1 (Q107)
ANY OTHER SIC CODE. . . . . 2 (Q113) [ ] 37

107. During the reference pay period, were there any self-employed individuals, that is, independent contractors who were working as an integral part of (this/these) establishment(s) even though they were not employed by the establishment(s).

YES. 1 (Q108)
NO . . . . . 2 (Q113) [ ] 38

108. How many independent contractors did you have working with (this/these) establishment(s) during the reference pay period? Include full- and part-time contractors. [ ][ ][ ][ ] 39 40 41 42

ONE CONTRACTOR. 0001 (Q109)

OR

NUMBER OF CONTRACTORS: _____ (Q110)

DON'T KNOW. 9998 (Q113)

109. Did this contractor earn $6.00 an hour or less from his or her work after expenses?

YES. 1 (Q111)
NO 2 (Q113)
DON'T KNOW . . . . . 8 (Q113) [ ] 43

110. Of these independent contractors, how many would you estimate earned $6.00 an hour or less from their work after expenses? [ ][ ][ ] 44 45 46

NUMBER OF CONTRACTORS: _____ (Q111)
NONE 997 (Q113)
DON'T KNOW 998 (Q113)

111. About how much did the typical independent contractor earning $6.00 an hour or less, earn after expenses during the reference pay period? [ ][ ][ ] 47 48 49

$ _____
PER
DAY. 1
WEEK 2
MONTH. 3
DON'T KNOW 998

112. About how many hours did the typical contractor work on matters associated with your business during the reference pay period? [ ][ ][ ] 50 51 52

HOURS: _____
PER
DAY. 1
WEEK 2
MONTH. 3
DON'T KNOW 998

113. What were the gross sales or receipts of (this/these) establishment(s) for the October through December 1979 quarter? (IF NOT AVAILABLE BY QUARTER BUT AVAILABLE FOR OTHER UNITS OF TIME, OBTAIN FOR OTHER UNIT. 1st CHOICE WOULD BE CALENDAR 1979; 2nd, CALENDAR YEAR TO DATE; 3rd, FISCAL YEAR TO DATE; 4th, MONTH OF OCTOBER; 5th, MOST RECENT FISCAL YEAR.)

SALES FOR OCTOBER-DECEMBER QUARTER: $_____
OTHER (SPECIFY $): $_____
(SPECIFY TIME PERIOD): _____
DON'T KNOW. 999999998

[ ][ ][ ][ ][ ][ ][ ][ ][ ] 53 54 55 56 57 58 59 60 61

114. How much of an increase in sales could (this/these) establishment(s) have handled during the October through December quarter given your physical capacity at that time? In your opinion, could you have handled:

a very large increase, ... 1
a large increase, ... 2
a moderate increase, ... 3
a small increase, or 4
no increase? 5
DON'T KNOW . . . . . 8 [ ] 62

115. How many employees both full and part time (does this/do these) establishment(s) currently employ?

NUMBER OF EMPLOYEES: _____ 63 64 65 66 67

116. Two years from now, do you expect that the size of your work force will have increased, decreased or stayed the same? (WORK FORCE IN ESTABLISHMENTS IN 1A)

INCREASED. 1 (Q117)
DECREASED. 2 (Q117)
STAYED THE SAME. 3 (Q118)
DON'T KNOW 8 (Q118) 68

117. How many (fewer/more) employees do you think you will have?

EMPLOYEES: _____ 69 70 71 72
DON'T KNOW 9998

118. (Is this/Are these) establishment(s) affiliated with any local business organization?

YES. 1
NO 2 73

BOX D

That completes the interview. Because one of the goals of our study is to measure the effects of changing economic conditions over time, we will be contacting your organization again next year. We will be sending you in a few weeks, some information on the study that you may want to retain in your files. Thank you very much for the time and cooperation you have given us.

INTERVIEWER: FILL OUT AFTER COMPLETION OF INTERVIEW

Q REFERRED TO RECORDS WHEN ANSWERING:

	YES	NO	DON'T KNOW
SECTION A.	1	2	8
SECTION B.	1	2	8
SECTION C.	1	2	8
SECTION D.	1	2	8

74 75 76 77

COMMENTS: _____

TIME ENDED: _____ AM PM

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